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Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

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The Engravings consist of 1. Dissections of Plants of the Class Cryptogamia, delineated from Nature, by Mrs. A. Ibbetson, in two Plates. 2. Mr. E. Lydiatt's Smicrologometer, for ascertaining the Tenacity of Metals, and Strength of Threads of Silk, Cotton, Linen, &c. 3. A Dissection of a Flower, in a 4to. Plate: and 4. A Branch of Laburnum, with a Section considerably magnified; all delineated from Nature by Mrs. Agnes Ibbetson. 5. Closure and draining Bricks, by J. Stephens, Esq. 6. Method of constructing a temporary Rick for securing Corn in wet Weather, by W. Jones, Esq. 7. An improved Dibble for planting Acorns in Bushes, by Mr. C. Waistell. 8. A Potato, with the Method of taking Sets from it for preventing the Curl, by Mr. T. Dickson. 9. Diagram illustrative of Electric Attractions and Repulsions, by J. C. Delamétherie. 10. Diagram illustrating the Law of Evaporation, by Honoré Flaugergues. 11. Different Modes of constructing the Breech of a Gun, so as to make it throw the Shot close, or scattering, by a Correspondent. 12. An improved Scarificator, by Mr. John Fuller.

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MAY, 1812.

ARTICLE I.

On the Fructification of the Plants of the Class Cryptogamia. In a Letter from Mrs. Agnes Ibbetson.

To Mr. NICHOLSON.

SIR,

IN my last letter I showed the dissection of fresh-water plants, endeavouring in a particular manner to mark the effect produced in different vegetables by the more or less water which surrounded them in their growing state; and proving, that those large divided air vessels are to be found in fresh-water plants alone; the vessels decreasing as the ditch, in which they were in the habit of growing, approached more to boggy or wet ground instead of water. This is truly exemplified in what I have called the half-water plants: there are however a few exceptions to this rule; and, since I last wrote, one has occurred to me in the *arum*, which, though long removed to tolerably high ground, still retains its immense air vessels. But in comparing fresh-water plants with marine plants, the alteration and transition is excessive. Instead of large bladders of air, circular wood vessels

Difference of boggy and water plants.

Marine plants.

vessels, and the strongly marked vital line, I find an extremely compressed formation, so delicate and fine, that it is very difficult to comprehend its uses and capabilities.

Difficulties of the study of the cryptogamiæ.

But before I enter on the subject of the cryptogamian plants, I must say a few words in vindication of an undertaking, that may appear to many (considering the number of learned men that have written on the subject) so little necessary. Linnæus might be said to select all the difficulties of botany, and unite them in one class. Yet though the various genera differ so much from each other, they are certainly most properly arranged, since they carry strong marks of internal resemblance; of which, I doubt not, that great master had a perfect knowledge. The very difficulties of the study appear to have constituted part of the charm, which has tempted such numbers to seek for, and try to understand this class of plants. Hence we find so many masters, who have dedicated their whole lives to the perfecting the knowledge of one single genus of the cryptogamian plants. This being the case, will it not be construed into extreme vanity in me, to select such a subject? Yet the plan I have formed cannot be complete without it; and there is certainly one part, that has not yet been touched: neither Gmelin, Dillenius, nor Stackhouse has dissected the interior of these plants. No master has proceeded farther than selecting and describing them, and giving their habitats: all which is so admirably shown in that incomparable work, the joint labours of Dr. Smith and Mr. Sowerby. This part therefore, "the dissection of the interior of plants," I may venture to appropriate; and should I, in the review I mean to take of the whole class, contradict the assertions of any of the great men I have before mentioned; it will, I hope, be considered, that I only venture to do it from possessing more powerful means of magnifying than they did, rendering the objects clear and luminous; which constant study has taught me the means of doing with effect.

Few masters have dissected cryptogamian plants.

Importance of the line of life in indentifying plants.

If the vital part of a plant was productive of no other consequence than that of marking its existence, I should not so continually have pressed it on the notice and attention of the public: but it is the centre from which every other line must take its rise, it is the point which must cer-

tify

tify the identity of every other part. Thus, by tracing the vital line, the seed, the bud, the flower, the radicle, are all ascertained and proved. The interior vessel of the pistil is formed by this line alone, which, being a cylinder, conveys the mixed juices to the seeds. I have shown this before in all other plants, but it is to the cryptogamia I trust for completing the proof of all I have before advanced on this subject. Its admirable conformity in the direction of its vessels; its agreeing in all points of the fructification, not only with each genus of this class, but with all others; establishes (in my opinion) the truth of both in an eminent degree. I have said also, that the wood conveyed the peculiar juice for the formation of the pollen: and I trust the 24th class will exemplify the truth of the fact; for in the interior of these plants, and by the direction of those two vessels, will botanists be alone able to discriminate and identify the stamen and pistil of these diminutive vegetables. To prove this I shall first show the formation of marine plants; and then endeavour to explain the fructification of the cryptogamia in general, and mark, by the direction of the vessels, which is the stamen and pistil of each plant. Stamen and pistil traced by lines.

Though the marine plants, (such as the fuci and ulvæ), have the appearance of stems, yet in the greatest part of these plants it is appearance only. When subjected to the strongest magnifiers, placing a thin cutting of each in a solar microscope, they present exactly the same picture, except that the stalk is thicker and more compressed than that which is properly named leaves. As the sea weeds are almost without vessels, (at least have only two or three in a large surface) they have of course no liquid of the nature of sap to diffuse into different parts of the plant. This is proved by one part drying and dying, though the adjoining part is immersed in water; the former not benefitting by this, as it has no vessels, that can convey the moisture; which, I suppose, is given merely by pores at the surface, and passes not from one bleb to the other. Interior formation of the fuci.

The fuci might be properly divided into thin and thick fuci: The 1st, as the dulse, the palmatus, coccineus, and all of this kind, consists of that transparent and almost invisible skin doubled; which, in all common leaves, makes a Division of the fuci. Structure of the thin fuci.

part

part of the cuticle of each side. But, what is most extraordinary, this skin, instead of being without, is in the interior; and, if you lay the dulse (or any other of this kind) on a glass, and scrape it very carefully on both sides with a knife, you will find all the exterior rubbed off, and nothing will remain but the almost invisible skin. This roughness I take to be the bark, it is most regularly placed in diamonds, (see Plate I, fig. 1, dulse unscraped); and answers well to the same matter, either within or without the transparent skin, in almost all the cryptogamian plants of every different genus. We trace it in the roughness at the exterior of the lichen, under the clear skin in the thick fuci, and so on to most of the class. But in the thicker fuci the transparent skin is on the exterior; and when it is taken off, and also the thin rough bark, the consistence of the matter underneath differs greatly from that of the thin fucus. It is so glutinous, so capable of distention, that, if drawn out or pressed, after being laid in fresh water, it may be reduced to what appears its original formation; that is into cylinders or strings, formed as at fig. 2. They cannot properly be called vessels, for they certainly appear not to convey any liquid; but to be a glutinous mass, in this shape. On examination of all the different fuci I could procure, I could find only these two vessels in each plant: 1st. The line of life which passes to the pistil, and afterward ties the seeds together: 2d. The wood vessels, which run directly to the male, and convey not only its peculiar juice, but the spiral wire that produces its motion. To make this plain, to enable any person to discover immediately both the vessels, and the stamen and pistil, I shall give this easy rule: When the line of life appears in the interior of a plant alone, and no wood vessels are found, it is certain, that the male is in a different plant. When both line of life, and wood vessels, are found joined together, you may be sure to find the fructification in the same flower. And when both vessels are found, but separate, it is always a sign, that the stamen and pistil are in different parts of the same plant. This law holds good in all the cryptogamian plants, nor have I ever found it vary.

Formation of the interior of the thick fuci.

Only two vessels in the fuci.

Rules for finding the vessels, and the stamen and pistil.

Mistake corrected.

I must now apologize for a mistake I have made in my former

former letter, in saying, that the spiral wire was found only in the conferva of all this class of plants. But I had so often sought it in all the sea weeds, and in the lichens, without discovering the smallest traces of it; that I felt convinced it was not there. As it is found in the male plant only, few would undertake the labour that is necessary to find it. In the mosses however it abounds, and in the woody part of the lichens also; and particularly distinguishes the male plant, whether single or joined to the female, by its never-ceasing motion. So violent is it often, that it is with great difficulty that it can be confined sufficiently for inspection, especially when first taken from within the flower. This alone makes a very distinguishing mark of the male in all the cryptogamia, for the female is quite inert. When its size has permitted me to take out the spiral wire, it leaves the rest of the plant perfectly quiet. I have therefore in various cases absolutely ascertained, that it is this alone which is the cause of motion in all plants.

Motion the sign which distinguishes the stamen.

That the fructification should have been continually mistaken by those, who had no other rule but mere guess to which they could apply for the discovery, cannot appear astonishing to any one; since, not knowing the interior formation, they could neither appeal to its analogy, with respect to other plants; nor to any means except the appearance and figure. But, as I have long been accustomed to be led up to the female by a peculiar line; I sought this in all the cryptogamia, and directly found it. It was not indeed quite so easy to discover in the male plant; but remembering, that the wood in all other plants formed the stamen; and that I had every reason to be convinced, as there was a peculiar juice for the formation of the pollen, there must be some vessel to convey this: this idea excited my diligence in seeking it, and I soon succeeded; and not only found the wood vessel meandering from male to male, but discovered, that in this class the spiral vessels always accompanied it as in every other plant. It is of extreme consequence to trace these lines in the cryptogamiæ; since without them it is impossible, that any person can be assured, that the male and female, if separate, belong to the same plant; whereas the running of the wood vessels from part to part will

Why the fructification has been mistaken.

Search of the line leading to the male.

will quickly ascertain, whether it is the original or a parasite plant: if no wood vessel leads to it, it should be condemned at once.

Fructification of the fuci.

I shall now turn to the fructification of the cryptogamiæ; beginning with the sea weeds, but leaving out the conferva; which of itself would nearly occupy a letter. The fructification of the fuci in general is exemplified in a specimen of the fucus serratus; which I shall just describe. A jellylike mass, with seeds bearing granules, and external papillæ.

Fucus serratus described.

Though the apparent anomaly, that prevails in the fructification of the fucus genus, is acknowledged by all, yet this variety is more in appearance, than in reality, as I shall prove in a future letter. Whatever may be the difference in the formation of the marine plants, and in their means of receiving nourishment; in all the general lines of their fructification they differ not from all other plants. The line of life composes the female plant invariably, and is always to be known by its direction; and the seeds are tied by the same line. Hence it is easy to discover it; since, wherever a branch is going to shoot (in the thick fuci especially) if we seek the line from which the bud proceeds, it will directly point out the line of life, or vital mark. The wood vessels are always to be traced to the male plant, whether carrying sap or no sap: for these are those peculiar juices already mentioned, of a more oily nature and wholly destined to the formation of the pollen. Fig. 3 is the tubercle of the fucus serratus; CC is the line of life leading to it: fig. 4 is a circle under the tubercle, which has rays proceeding from it, to which the seeds are always attached, and to which, let them appear ever so much scattered, they are invariably fixed.

General analogy between the marine and other plants.

As to the male plant, it is certainly the pencilled vesicles on the frond as it is also in the vesiculosus and many others. When much magnified, they are very curious; in the first it is pitcher shaped, from out of which tubercle the powder proceeds. In the vesiculosus it is a sort of ring, in which the powder is formed, and worked into the hairs. In both the wood vessels meander from male to male, and the hairs (if prevented sticking on the frond) move much when breathed on, and when shaking out the powder from its filaments. See fig. 5, *ee* male, F the wood

Vesicles the male of the fuci.

wood vessel leading to it. The stamens are said to be permanent; but this is certainly not the case, since it is only once a year the powder is found in the hairs: but as the old ones remain a long time after they have performed their office, before they decay and fall off; it gives them the appearance of perpetuity.

The Ulvæ.

Though I at first intended to give a marine plant, yet being so thoroughly acquainted with the ulva crassata; I preferred showing its dissection. It is formed of a membranaceous frond, with minute thick set tufts of branched filaments jointed, and beaded; the female being the ball; and the top, which is perforated, constituting the pistil, (see fig. 8). Under the tufts, G, fig. 7, the seeds are imbedded in regular order, each holding by the line of life; see GG the pointed filament, which proceeds from the pistil, and the wood vessels of which run up round it, and serve as a cuticle to it; showing themselves also on each side of the capsule and its stem as seen at HH, fig. 8. When the plant is first taken out of the water, and gently dried, if its pollen is ripe, and the hairs stick not on the frond; when breathed on they move more than the males of the fuci. I have seen them rising and falling with a constant succession of motions, which gave to the plant an appearance of life difficult to describe; but if too wet, or too dry, they move not. I found much of this ulva in a pond at Bellevue, near Exeter. The ulva pruniformis much agrees with this; there are certainly two sorts; one resembling in its fructification the lemna, and the other the crassula; but, as I got it twice only, and then rather in a dry, decayed state, I was fearful of making some mistake, if I should attempt to review it.

Fructification of the ulvæ.

The Musci.

It is very painful to me, to be obliged to contradict those, whose superiority I so gratefully acknowledge; as every botanist must the uncommon labours of a Dillenius, a Michelli, or a Gmelin: yet I cannot but differ from them respecting the fructification, which I would thus describe. The

Fructification of mosses.

The flower of the moss standing on a long stalk, and having its male and female in the same flower, being a capsule on a peduncle, sheathed at the base, with its seed vessel in the interior, the pistil standing up in the middle; the veil investing the fringes, which are truly the male part of the plant, and keeping them close, till the powder of the pollen is ripe, then both veil and lid fall off, the fringe spreads, and as soon as the drop appears on the pointal, the inner fringe draws over it. Then by breathing on it (when under a strong magnifier) any person can convince themselves, that these hairs are the males, since they throw out the pollen from every spray, till the top is covered with its powder. But as the inner fringe stands up in a pinnacle, the powder generally falls under on the stigma, by which means it is not so conspicuous, and is soon dissolved by the liquid of the pistil, and thence carried to impregnate the seeds. The outer fringe has from 4 to 32 teeth, which are either reflected, straight or twisted, triangular, spear shaped, blunt, or sharp; while the inner fringe is much finer, either closely adhering to the outward, or joined to it by threads from its inner sides; but which ever way it is formed, it has powder, which works out from the interior of the fringes. Many have between the hairs little balls on foot stalks, out of which proceeds the powder; others a sort of division up the hairs, which, when moved, gives out the dust, so that the inner fringe always appears variously jagged. Nature seems to have formed the lid to keep the fringe together, and prevent the hairs throwing out the powder, ere the seeds are ready to receive impregnation, or the liquid of the pointal to dissolve the pollen. It is these beautiful provisions of nature, that should be so closely watched. Who can behold all the exquisite contrivance displayed in the formation of these fringes, and not be convinced, that they were intended for some important purpose? Never is such perfect mechanism seen without it is designed to produce some great effect. The sight directly excites my mind to discover the use; nor do I allow myself to pass on to another subject, till I have studied hard to find out the cause.

Fringes the male part of the plant.

Description of the fructification.

The seeds are numerous and spherical, and all tied together by a line, which is the line of life. The wood vessels constantly

constantly lead up to the capsule, in which they form stripes. That which used to be called the male is a cryptogamian plant, found in all these diminutive vegetables, and taken generally for the stamen of the mosses, filices, lichens, and others: for, as it grows always, and has the appearance of powder, the mistake was very natural to those who knew no law, by which a parasite plant could be distinguished from the identical plant on examination. Fig. 9 is the capsule; I, its interior: fig. 10, is a more highly magnified view of the outer fringe, K, and the inner fringe, L: figs. 11 and 12 are the pitcher-shaped leaves. Most mosses, when they first shoot, require much water; and there being a quantity of spiral wire in the leaves, they easily draw into this shape, and for some time retain water in each leaf by the contraction the moisture occasions. That it is the spiral wire, that passes up the capsule in the wood vessels, is plainly shown in the stripe that accompanies this part, and is more strongly evinced in the figure of the tortula subulata. When the upper case stops some way below the seed vessel, the stripe leaves the outer case and runs up the under, in the shape of a corkscrew, to form both fringes. See Sowerby's admirable print, which is very exact. Vol. 16, p. 1101. Both fringes move, and both must concur in the office of the male, since the spiral is worked to and fro from the outward to the inner vessel repeatedly; and is seen in the microscope to contract and dilate at the bottom of the capsule, as I have marked at N N, fig. 9. In the polytricha, that which is supposed to be the male plant has certainly not only stamens, but a pistil, and is of itself a completely distinct plant; the middle of which opens, and shows the pointal, while the teeth around unclose at the edges, and discover the pollen. In all that I am acquainted with this is the case, but I know only four; it is not often I could find the plant called the male, and then they were perfectly divided, having their own stem and root.

Some mosses may have the male in a different plant.

As to the mosses that have no apparent fringes, there may be some having the male flower in a different plant: but, if I may be allowed to say what I have often experienced in many cases, when the fringes are not to be found in the usual place, I seek it in the lid or veil, where I seldom miss finding

finding it. They are so delicate, that the smallest touch breaks them. I do not however deny, that there may be some gymnostoma thus unsupplied; nature possesses so much variety of form, but then it is not generally shown in these points: there is a strict conformity in all that concerns the fructification of plants, that teaches us to expect a change less in these matters than in any other parts. Besides, they might be bent or broken. I think I have dissected the *g.* viridissimum, and if it was the plant (and it concurred in every other point) it has a very narrow rim of inflected teeth, which grew dark as the powder ripened. The polytrichum commune is very curiously formed at the bottom of the capsule; the manner in which the spiral is laced displays a mechanism most wonderful; if it was possible to understand the whole management.

The Filices.

Fructification of the ferns.

The general structure of the fructification of the ferns is as follows. The scale or calyx is not often found. It springs out of the leaf, opening on one side, and is different from the cover. The wood vessel and line of life, forming together, run up to each set of flowers, which are dispersed in parallel lines oblique to the midrib, commonly in one row on each side of it, but sometimes the row is double. Under the cover, usually supported on little foot stalks, are the flowers, encompassed by an elastic ring, which is really the male part of the plant. When the seeds are ripe, the impregnating cord springs and moves with every change of temperature, till it has shaken out all the powder to be found in it. The capsules then burst, the seeds disperse from the force of the confined membrane within the seed vessel, which, having the seeds fastened to it, and being coiled up in a manner adverse to its form, (as the spiral wire within it grows stronger) it struggles to get free, which it does at last by bursting the capsule, and throwing off the seeds to a distance; in the same manner as it does in the spirting cucumber, and in many other plants of that kind. That the elastic line that covers the apparent basket is really the male part of the plant, is easily proved. Let any one place the cyathea fragilis under a strong magnifier about the

the time the male powder is ripe. After observing the capsules to be covered on one side with a white shining mixture, they will soon see this turn to a pale green, from the powder which falls on it from the handle or elastic ring. The manner in which the pollen is given out is as curious as any part; for the ring contracts and dilates alternately, till it has yielded all its dust. Nor is there the least fear of taking the pollen for the seeds, the one being brown, the other almost white. In the month of September this mechanism is very plain in the asplenium scolopendrum; I have seen the male so difficult to confine from the eternal motion of the cord, that without a pair of pincers it was impossible to fasten it within the field of the microscope. Sometimes the fructification of this powder is in spikes, and then the flowers are contained within a case, as in the equisetum sylvaticum. There the male and female have been perfectly guessed; the capsule, which holds the seeds, being the pistil; while the agitated part attached to it is the stamen, and which may really be said to fly from the glass. I have seen them, when first thrown on the paper, move about like a worm, and if a drop of water is placed near them, the filaments gather round the capsule, as if to defend it; beating the anthers against it, till it is completely covered with powder. It has 4 filaments to each female.

Great agitation of the male plant.

Thus then we may lay down three rules for discovering the male plant: 1st, the leading up of the wood vessel to the part either with or without the line of life, as the male is or is not joined to the pistil. 2d, the constant motion of the filaments and anthers, when giving out their powder; which agitation belongs only to the male, for the female is perfectly inert. 3d, the stamens being almost constantly in the shape of hairs, which will lead a student at once to examine every thing in the cryptogamiæ that bears this appearance.

Rules for discovering the male.

The fructification of the filices is seen in Pl. II. Fig. 1, *oo*, the joint wood vessel and line of life leading up to the fructification, in the leaf of the scolopendrum vulgare: fig. 2, P, the capsule with the pointal; Q, the elastic line or stamen: fig. 3, the seeds tied by the line of life. The fructification of the equisetum sylvaticum, palustre, and arvensis,

arvensis, for they exactly resemble each other: fig. 9 the target carrying the flowers: fig. 10, an interior view of the same cut through the middle: fig. 11, the pistil and capsule bearing its seed, with 4 stamens attached to it by their filaments, one of which is shown still more magnified at fig. 12. I was proceeding to the agarics, but my letter already appears so long, that I shall leave these for my next communication, and join to them the lichens, jungermanniæ, and marchantiæ.

I am, sir,
Your obliged servant,
AGNES IBBETSON.

Cowley Cot.
March the 8th, 1812.

Supposed males of certain mosses.

P. S. I think it right, however, to add the three males of the mosses, which I have found, dissected, and exposed to very great magnifying powers. See fig. 7, which, like a number of others, proved merely a collection of leaves: fig. 8, which showed a sort of pistil in the middle concealed and covered by the stamen: and figs. 5 and 6, which appeared the male of a polytrichum, but were certainly a complete flower with both stamen and pistil. The perfection and exactness of Mr. Sowerby's drawings no one would venture to contradict, and I mean not in any manner to do so, I have too many opportunities to admire the perfect likeness of each object. All that I would wish to suggest is, that the plants taken for males in the mosses are plants of the same genus, and having both male and female, which by dissection may be found. To prove this, there are many arguments, most strong and powerful. That nature should have formed all this beautiful apparatus for nothing; that these exact and regular fringes, thus exquisitely formed, should be made to bend over the seed vessel at a certain time, and rub out a powder: that the veil should remain, without any reason, a stipulated time, then quit it, for as little apparent cause; is not like her general arrangements. But, on the contrary, that nature should have placed all this spiral wire in the fringes, that its motion, might rub out the pollen from the teeth: that the veil should remain on, to keep the males from moving, till the seed

seed is fit to receive impregnation; that it should then fall off, and the fringe bend over the pointal, to mix the pollen with the juice of the pistil; and, to prevent the powder of the stamen from being lost in the seeds, that a thick curtain should be drawn between, to give time for the melting of the powder in the sweet juices of the pistil; all this is exactly conformable to the process in every other flower, and analogous to the proceedings of every other plant. But this is not all: that in all the rest of the cryptogamiæ the males should be distinguished for excessive motion, and yet in the mosses alone be different, is not to be credited. Besides, when the supposed male flower is found; it is often not one to ten thousand females; and considering, that much powder must be lost in attaining the pistil, nature would have provided a quantity, as it does in every other case I am acquainted with, where the male flower is separated from the female. These are all strong reasons for believing, that the male plant has been generally mistaken. But there is another source of errour admirably suited to mislead. There is a species of animalcule, which lays its light green eggs very often in some species of mosses; and generally chooses the upper leaves, whence they open to the stalk. These are so like pollen, that it is only keeping them till they hatch, that can prove what they are. I have been twice so deceived. I have added a dissection, at fig. 4, of the stem of the moss, to show the manner in which the spiral wire runs from leaf to leaf at z; and to show the ball, round which it winds at every leaf, thus running up the midrib.

Source of errour in the eggs of animalcules taken for pollen.

II.

Trigonometrical Formulæ for Sines and Cosines. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

FROM your favourable reception of the Trigonometrical Formulæ, which I had the honour of communicating, and which

which appeared in your Number for February last, I have been encouraged once more to trouble you with a few miscellaneous results, indeed, yet curious.

By the common trigonometrical resolutions of sines and cosines we have: If $\pi = 3{\cdot}1415$ &c.

$$\text{Sin. } A = A \left(1 - \frac{A^2}{\pi^2}\right) \left(1 - \frac{A^2}{(2\pi)^2}\right) \left(1 - \frac{A^2}{(3\pi)^2}\right)$$

$$\left(1 - \frac{A^2}{(4\pi)^2}\right) \times \text{ \&c. to infinity.}$$

$$\text{Hence } A = \frac{\text{Sin. } A}{\left(1 - \frac{A^2}{\pi^2}\right) \left(1 - \frac{A^2}{2^2\pi^2}\right) \left(1 - \frac{A^2}{3^2\pi^2}\right) \cdot \text{ \&c.}}$$

Let now $A = \frac{m}{n}\pi$. And $\therefore \pi = \frac{n}{m} \cdot \text{sin.} \left(\frac{m}{n}\pi\right) \times$

$$\frac{n^2 . (2n)^2 . (3n)^2 . (4n)^2 . \text{ \&c.}}{(n^2 - m^2)\ (\overline{2n}^2 - m^2)\ (\overline{3n}^2 - m^2)\ (\overline{4n}^2 - m^2) . \text{ \&c.}} = \frac{n}{m} . \text{ sin. } \frac{m}{n}\pi .$$

$$\frac{n. \quad n. \quad 2n. \quad 2n. \quad 3n \quad 3n. \quad \text{\&c.}}{(n-m)\ (n+m)\ (2n-m)\ (2n+m)\ (3n-m)\ (3n+m) . \text{ \&c.}} \cdot \{1\}$$

Again

$$\text{Cos. } A = \left(1 - \frac{A^2}{\left(\frac{\pi}{2}\right)^2}\right)\left(1 - \frac{A^2}{3^2\left(\frac{\pi}{2}\right)^2}\right)\left(1 - \frac{A^2}{5^2\left(\frac{\pi}{2}\right)^2}\right) . \text{ \&c.}$$

$$= \frac{(n-2m)(n+2m)(3n-2m)(3n+2m)(5n-2m)(5n+2m) . \text{ \&c.}}{n \ . \ n \ . \ 3n \ . \ 3n \ . \ 5n \ . \ 5n . \text{ \&c.}} \ \{2\}$$

In $\{1\}$ let $\frac{m}{n} = \frac{1}{2}$. $\therefore$ sin. $\frac{m}{n}\pi = 1$, and we get

$\pi = 2 . \frac{2 . 2 . 4 . 4 . 6 . 6 . 8 . 8 . \text{ \&c.}}{1 . 3 . 3 . 5 . 5 . 7 . 7 . 9 . \text{ \&c.}}$. Which is Wallis's expression. This way of deducing it is however far shorter and more direct than the usual way (see Woodhouse's Trigonometry, where, however, he does not seem to have bestowed much attention on this part of his subject).

In $\{1\}$ let $\frac{m}{n} = \frac{1}{4}$ $\therefore$ sin. $\frac{m}{n}\pi = \frac{1}{\sqrt{2}}$, and the form becomes

$$\pi = 2\sqrt{2} \times \frac{4 . 4 . 8 . 8 . 12 . 12 . 16 . 16 . \text{ \&c.}}{3 . 5 . 7 . 9 . 11 . 13 . 15 . 17 . \text{ \&c.}} \qquad (a)$$

Let $\frac{m}{n} = \frac{1}{6}$ $\therefore$ sin. $\frac{1}{6}\pi$ being $= \frac{1}{2}$ form $\{1\}$ becomes

$$\pi = 3 \cdot \frac{6 \cdot 6 \cdot 12 \cdot 12 \cdot 18 \cdot 18 \cdot 24 \cdot 24 \cdot \&c.}{5 \cdot 7 \cdot 11 \cdot 13 \cdot 17 \cdot 19 \cdot 23 \cdot 25 \cdot \&c.} \qquad (b)$$

Let $\frac{m}{n} = \frac{1}{3}$, and form $\left\{1\right\}$ becomes

$$\pi = \frac{3\sqrt{3}}{2} \cdot \frac{3 \cdot 3 \cdot 6 \cdot 6 \cdot 9 \cdot 9 \cdot 12 \cdot 12 \cdot \&c.}{2 \cdot 4 \cdot 5 \cdot 7 \cdot 8 \cdot 10 \cdot 11 \cdot 13 \cdot \&c.} \qquad (c)$$

In the same way by making $\frac{m}{n} = \frac{1}{10}$, or $\frac{1}{20}$, we find

$$\pi = \frac{5(\sqrt{5}-1)}{2} \times \frac{10 \cdot 10 \cdot 20 \cdot 20 \cdot 30 \cdot 30 \cdot 40 \cdot 40 \cdot \&c.}{9 \cdot 11 \cdot 19 \cdot 21 \cdot 29 \cdot 31 \cdot 39 \cdot 41 \cdot \&c.} \qquad (d)$$

And

$$\pi = 5\left\{\frac{\sqrt{5}+1}{\sqrt{2}}\sqrt{5-\sqrt{5}}\right\} \cdot \frac{20 \cdot 20 \cdot 40 \cdot 40 \cdot 60 \cdot 60 \cdot \&c.}{19 \cdot 21 \cdot 39 \cdot 41 \cdot 59 \cdot 61 \cdot \&c.} \qquad (e)$$

and so on, whenever sin. $\frac{m}{n}\pi$ can be found in algebraic terms, as if $\frac{m}{n} = \frac{2}{17}, \frac{1}{17}$, &c.

Let us now take form $\left\{2\right\}$, and for $\frac{m}{n}$ write $\frac{1}{4}$. Now cos. $\frac{1}{4}\pi = \frac{1}{\sqrt{2}}$. $\therefore \sqrt{2} = \frac{1}{\cos.\pi}$, and the form becomes

$$\sqrt{2} = \frac{4 \cdot 4 \cdot 12 \cdot 12 \cdot 20 \cdot 20 \cdot 28 \cdot 28 \cdot \&c.}{2 \cdot 6 \cdot 10 \cdot 14 \cdot 18 \cdot 22 \cdot 26 \cdot 30 \cdot \&c.}$$

$$= \frac{2 \cdot 2 \cdot 6 \cdot 6 \cdot 10 \cdot 10 \cdot 14 \cdot 14 \cdot \&c.}{1 \cdot 3 \cdot 5 \cdot 7 \cdot 9 \cdot 11 \cdot 13 \cdot 15 \cdot \&c.}$$ which is an expression due (if I remember rightly) to Euler.

In form $\left\{2\right\}$ for m write $\frac{1}{2}m$, and it becomes

$$\text{cosin. } \frac{m}{2n}\pi = \frac{(n-m)}{n} \cdot \frac{(n+m)}{n} \cdot \frac{(3n-m)}{3n} \cdot \frac{(3n+m)}{3n} \cdot \frac{\&c.}{\&c.}$$

Let $m = 1$, $n = 3$. $\therefore$ cos. $\frac{1}{6}\pi$ being $= \frac{\sqrt{3}}{2}$ we get

$$\sqrt{3} = 2 \cdot \frac{2 \cdot 4 \cdot 8 \cdot 10 \cdot 14 \cdot 16 \cdot 20 \cdot 22 \cdot \&c.}{3 \cdot 3 \cdot 9 \cdot 9 \cdot 15 \cdot 15 \cdot 21 \cdot 21 \cdot \&c.} \qquad (f)$$

Let $m = 4$, $n = 5$. $\therefore \cos. \frac{4}{10}\pi = \cos. \frac{2}{5}\pi = \frac{\sqrt{5}-1}{4}$ &c. :

$$\sqrt{5} = 1 + 4 . \frac{1 . 9 . 11 . 19 . 21 . 29 . 31 . 39 . 41 . 49 . \&c.}{5 . 5 . 15 . 15 . 25 . 25 . 35 . 35 . 45 . 45 . \&c.} \quad (g)$$

In the same way as Euler's theorem, $\frac{A}{2} = \sin. A - \frac{1}{2} \sin. 2 A + \frac{1}{3} \sin. 3 A -$ &c. is deduced, we may obtain the following

$$\left.\begin{array}{l} \cos. A + \cos. 3 A + \cos. 5 A + \&c. \text{ to infinity, always} = 0 \\ \cos. 2 A + \cos. 4 A + \cos. 6 A + \&c. \text{ always} = -\frac{1}{2} \end{array}\right\} \quad (h)$$

and $\therefore \cos. A - \cos. 2 A + \cos. 3 A - \&c. = \frac{1}{2}$ as may also be had by differencing Euler's series.

Again, if $e = 2{\cdot}7182818$ &c. we find

$$\left.\begin{array}{l} 2 . \cos. \frac{A}{2} = e^{\cos. A - \frac{1}{2} \cos. 2 A + \frac{1}{3} \cos. 3 A - \frac{1}{4} \cos. 4 A + \&c.} \\ \text{and } 2 . \sin. \frac{A}{2} = e^{-(\cos. A + \frac{1}{2} \cos. 2 A + \frac{1}{3} \cos. 3 A + \frac{1}{4} \cos. 4 A + \&c.)} \end{array}\right\} (i)$$

$$\text{Again } \frac{\pi - A}{2} = \sin. A + \frac{1}{2} \sin. 2 A + \frac{1}{3} \sin. 3 A + \&c. \quad (k)$$

$$\text{And } \frac{\pi^2}{6} - \frac{\pi A}{2} + \frac{A^2}{4} = \cos. A + \frac{1}{2^2} . \cos. 2 A + \frac{1}{3^2} . \cos. 3 A + \frac{1}{4^2} . \cos. 4 A + \&c. \quad (l)$$

$$\text{And again } \frac{A}{2}(\pi - A) = \frac{(\sin. A)^2}{1^2} + \frac{(\sin. 2 A)^2}{2^2} + \frac{(\sin. 3 A)^2}{3^2} + \frac{(\sin. 4 A)^2}{4^2} + \&c. \quad (m)$$

These last theorems are so easy of deduction, that I have omitted their demonstrations for the sake of keeping within the compass of a letter.

I am, Sir,

Your most obedient humble servant,

March the 23d, 1812 ANALYTICUS.

III.

Inquiry concerning the Means of studying the Modern Analysis. In a letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

AS the object of your excellent Journal is the diffusion of scientific knowledge among all classes, I am sure you will not deem the present queries out of place; and if you will have the goodness to reply to them either by private communication, or through the medium of your publication, you will confer an obligation not merely on the individual who addresses you, but upon many others in the same circumstances as myself.

What books are necessary to the study of the modern analysis?

The object of my inquiry is this:—What elementary works should be perused by a person who wishes to become acquainted with what is usually termed "*the modern analysis*"? That one who resides in a Mathematical University should put this question may appear strange; but it is well known by many, who, like myself, have devoted a considerable portion of time to the study of mathematics according to the system adopted in this university,—that so little attention is paid to the *modern language* of science, that the most admired works of the foreign Mathematicians are a dead letter even to many of those, who are sufficiently familiar with the works of Newton and the ablest English philosophers.—Suppose then that a person is tolerably acquainted with pure *Geometry*, and with the *fluxional* calculus, what course of reading should he pursue, in order to qualify himself for the perusal of *La Place's Mécanique Céleste*?

As these observations are addressed to you by one who is an enthusiast in mathematical studies, but who knows of no other means of getting satisfactory information upon the subject of his inquiry, than that which he has adopted, an early reply would be extremely acceptable.

A. H. Z.

Answer.

Works recommended for the study of the modern analysis.

With respect to the books my correspondent inquires after, I would recommend, as the first and principal, the *Traité du Calcul Différentiel et Intégral* of Lacroix; which, with the qualifications he mentions himself as possessing, will be sufficient to give him a very complete notion of most of the branches of the *modern analysis*. He should, however, read with great attention, before he begins to look into the *Mécanique Céleste* of La Place, the *Traité de Mécanique Elémentaire* of Francœur, which is an excellent introduction to that work, and the *Mécanique Analytique* of La Grange, which is a work of the first rank in this department of science. If to these he joins the *Théorie des Fonctions Analytiques*, and *Leçons sur le Calcul des Fonctions*, by La Grange, he will be able to proceed, with great ease, in any undertaking of this kind, that he may wish to engage in; these being, as I conceive, all the most necessary and useful performances, that have hitherto appeared on the subject of what is more peculiarly called the *modern analysis*.

IV.

Experiment to prove, whether Water be produced in the Combination of Muriatic Acid Gas and Ammoniacal Gas. By John Bostock, *M. D., Vice Pres. of the Lit. and Phil. Soc. of Liverpool, and* Thomas Stewart Traill, *M. D., Secretary to the Society. Read before the Literary and Philosophical Society of Liverpool, and communicated by Dr.* Bostock.

To Mr. NICHOLSON.

SIR,

Mr. Murray's attempt to prove the existence of water in muriatic gas.

IN your Journal for February, Mr. Murray has related an experiment, which he performed on the mixture of muriatic and ammoniacal gasses, the object of which was to ascertain, whether, when the gasses were added together in the

the state of perfect dryness, the muriate of ammonia, formed by their mixture, contained water. A very obvious quantity of water was expelled from the salt, and it was argued, that this water must have formed a constituent part of the muriatic gas, for it is now agreed, that pure ammonia consists entirely of hidrogen and azote; and from the terms of the experiment it is supposed, that all moisture was removed from both the gasses, and excluded from every part of the apparatus. In your Journal for March, a correspondent, who signs himself A. B. C., undertakes to set aside the inference from the above experiment. This he does, not by showing that either of the gasses, or any part of the apparatus, contained water, nor by denying the existence of water in the result of the process as conducted by Mr. Murray, (for these points appear to be admitted) but by attempting to prove, that the muriate of ammonia had attracted moisture from the atmosphere, while it was transferred from the vessel in which it was originally formed, into the one to which the heat was applied; and to prove this he relates an experiment, in which newly formed muriate of ammonia attracted water, simply by being "removed through the atmosphere "into a dry tube."

The moisture said by a correspondent to have been attracted from the atmosphere.

The experiments are in themselves curious, and are at this time particularly interesting, as forming a part of the controversy respecting the constitution of muriatic acid. From these considerations Dr. Traill made a proposal, to which I very willingly assented, that we should in conjunction repeat the experiments of Mr. Murray and the correspondent; that we should especially attend to every circumstance, by which moisture might be excluded; that the muriate of ammonia formed should be heated, without being at all exposed to the air; and that the quantity of moisture, which it acquired from exposure to the atmosphere, should be accurately ascertained. Before we entered upon the process we resolved, that, provided no circumstance occurred to interrupt or defeat the experiments, the results, whatever they were, should be communicated to your Journal.

The experiments carefully repeated.

Every circumstance as to the cleaning and drying the different parts of the apparatus, and the providing of the necessary substances, being attended to, we commenced our operations

Preparation of the muriatic gas.

operations by the preparation of the muriatic gas. Two ounces of muriate of ammonia, in coarse powder, and which had been kept heated for two days, were mixed with 9 drachms by measure of sulphuric acid, of the specific gravity of 1·85, in a tubulated retort. The gas soon began to form, without the assistance of heat; and, after a considerable quantity had escaped, we received a portion of it over mercury. The gas was perfectly transparent and colourless, no moisture was perceived within the jar, and none was visible in any part of the retort; it was indeed observed, that some particles of the muriate of ammonia, which had lodged on the lower part of the neck of the vessel, remained perfectly dry at the end of the process. A quantity of muriate of lime, perfectly dry and pulverulent, was introduced through the mercury into the muriatic gas, and in this state it remained for 48 hours.

Preparation of the ammoniacal gas.

The ammoniacal gas was prepared by introducing into a retort equal weights of newly burned quick lime and muriate of ammonia, in the same state with that used above. By means of a lamp gas was expelled, and after a sufficient quantity had escaped, a portion was received over mercury. When the jar was become cold, a little dew was perceived on the upper part, which was very carefully removed by bibulous paper, introduced on the end of a wire. A considerable lump of dry quick lime was then placed in the gas, and was suffered to remain for 48 hours.

Mixture of the gasses.

At the end of this time we resumed our operations. Upon the closest inspection we could not perceive the least moisture in either of the gasses, or appearance of it in the jars; the lime and the muriate of lime were withdrawn, and it was observed, that the latter was to all appearance as dry as when it was first introduced. A flask, furnished with a ground stopper and bent tube, had 13 cubic inches of ammoniacal gas introduced into it over mercury, and to this was added 6 cubic inches of muriatic gas in successive portions. The flask was then entirely filled with ammoniacal gas, and the apparatus was left at rest for about an hour; it was coated, more especially at its lower part, with a fine frost work of muriate of ammonia. The stopper and tube were then introduced, and the flask was turned over, but so

Muriate of ammonia formed, and,

that

that the end of the tube was kept below the surface of the mercury, so as to exclude all communication with the atmosphere. The flask was then embedded in a charcoal furnace, and gradually heated, until it was softened. This process continued about an hour, when the muriate of ammonia was all sublimed into the neck of the flask, or into the commencement of the tube. When the salt was about half sublimed, a dew was observed to form at the upper part of the curvature of the tube, about an inch from the stopper. This dew increased, so that at one period it occupied a zone all round the tube of about an inch in width, and some globules of water were formed of about the size of a small pin's head. Towards the end of the experiment, as the heat increased, the dew was diminished; but when the tube was removed from the mercury a similar deposition of moisture was observed at the end, where it had been immersed in the metal. Before it was taken from the mercurial bath the tube had its aperture luted with wax, in order to exclude all communication with the atmosphere, which was farther ensured by a globule of mercury being lodged in the curvature of the tube; and, as soon as it was become cool, the flask was opened, a part of the salt scraped from the neck, and weighed as quickly as possibly. The quantity collected was 2·7 grs, and not more than a minute could have elapsed between its being removed from the vessel, and its weight being ascertained. It remained in the scale for 15 minutes; but although we thought that the index of the balance rather inclined to that side, no increase of weight could be positively asserted to have taken place. In order that a judgment may be formed of the delicacy of the instrument, we found it to turn with $\frac{1}{10}$ of a grain, when each side was loaded with 500 grains.

without having been at all in contact with the atmosphere, sublimed in the same vessel.

A dew formed,

and moisture deposited at the end.

Part of the salt taken out and weighed quickly,

and it gained no perceptible addition of weight by exposure to the air 15 minutes.

I am, sir,

Your obedient servant,

J. BOSTOCK.

Knot's-hole Bank, near Liverpool,

March the 26th, 1812.

V.

V.

Questions respecting a Passage in Mrs. IBBETSON'S *Account of the Water Lily. In a Letter from a Correspondent.*

To W. NICHOLSON, Esq.

SIR,

Ambiguity in Mrs. Ibbetson's account of the water lily.

IN the last number of your very excellent publication, there is a paper of Mrs. Ibbetson's, in continuation of her valuable discoveries in the minute anatomy of plants, in which I would, though with the greatest deference, point out an ambiguity, in my opinion of considerable importance. The passage I allude to is in the description of the structure of the water lily, page 243, where, after referring to "*a. a*, of fig. 1, Pl. VII," for a view of the *air vessels*, she says, that, lest the pith "should not be sufficient to prevent insects from entering into it, and choking up the air vessel, as soon as the plant sinks in the water, a quantity of hairs, which are placed in circles in the interior, rise, and, meeting in the centre, not only aid to keep out the water, but run through every insect, that ventures to approach." Mrs. Ibbetson then goes on to add, "I have often caught insects threaded on the hairs, but they are soon washed off.

Questions respecting it.

Now a question or two naturally arise on reading this observation.—1st, how do insects get *into* or even *near* the air vessels? or, 2dly, how can the water come at them, to wash them off, when these vessels are so entirely internal?

I doubt not that these questions can be most satisfactorily answered; but, certainly, Mrs. Ibbetson did not show her usual perspicuity in this passage.

Your insertion of this, or an answer to it, if possible in your next, will very much oblige me.—I conclude by sincerely thanking the lady, who is the occasion of this letter, for the high gratification, which her frequent communications have afforded—and with hopes, that she will persevere in her truly wonderful and interesting discoveries.

I am, sir,

Your much obliged, and very humble servant,

Poole, April the 4th, 1812. T. B.

VI.

VI.

The Stater of Philip, the Father of Alexander; or Remarks on the Purity or Standard of Gold: By Mr. J. Fabbroni, of Florence, Corresponding Member of the French Institute.*

NATURALISTS, perhaps on the authority of Pliny (1) †, are almost unanimous in the assertion, that native gold is never found perfectly pure, or free from all alloy, particularly of silver; and that the finest is scarcely from 0·875 to 0·917, that is from 21 to 22 carats. The gold dust brought from Africa is commonly within these limits. I have seen some at 0·927, or 22 carats and a quarter ‡; and lately there has been some at 0·958, or 23 carats, brought from Morocco to the mint at Florence. (In Tuscany the carat is divided into eighths.)

Native gold said never to be pure.

Gold dust from Africa.

It is probable, that in the early ages money was coined of native gold, in the state in which it was found; for there could be no inducement to incur the trouble and expense of refining it.

Ancient coins from native gold.

The most ancient gold coin known is supposed to be that of Battus IV, cast or struck at Cyrene, in Africa, in the time of Pisistratus. Its fineness does not appear to be known. Of all the Grecian coins found in our cabinets of medals the most ancient are the beautiful pieces of Philip, father of Alexander. This enterprising man, who from his infancy looked forward to ascend the throne of Macedon and become master of Greece, had the good fortune to find some rich gold mines, which he knew how to work to great advantage. Mount Pangæus furnished him annually to the amount of 6300000 Florence liri [£218750]. Hence he derived the most powerful instrument of the success of his political designs and military talents. Whether the gold of Philip underwent any particular operations, before it was sent

Most ancient known.

Oldest Greek, those of Philip.

His mines.

* Ann. de Chim., vol. LXXII, p. 25.

† The figures refer to notes by Mr. d'Arcet at the end of the paper.

‡ This gold is found chiefly in the country of Bambouck.

sent to the mint, is not known; but there is reason to believe, that it was employed in the state in which it was found*.

Assay of his stater.

Patin assayed a gold stater of this king, and found it 23 carats and a half fine, or 0·979: and, as it cannot be supposed, that his mintmen would have thought of purifying gold, to add afterward no more than a forty-eighth of alloy, we may presume, that the gold was found native of this fineness.

Addition of alloy to gold.

If alloy have been added to gold with a bad design, or with the erroneous idea of defraying the expense of coinage; it is a remedy that has degenerated into fraud, and has no limits. If alloy have been added with the design of rendering the coin harder, it is a useless idea. Neither of these motives could have induced Philip to adopt the practice, because the source of his gold was abundant, and he was desirous of appearing generous; so that he would have coined his money of pure gold, if he had thought it necessary to refine it; or he would have added more alloy, if policy had suggested to him, not to employ it in the virgin state, as it came from the mine (2). It would appear therefore, that nature furnished him with gold at 23 carats and half, or 0·979, as it is in his coin; unless there were an errour in the assay of Patin, which deserves therefore to be verified.

Philip used his gold native.

A stater lately found.

The chevalier Fossombroni, a very celebrated mathematician, digging the foundations of a house near Arrezzo, found a stater of Philip in very good preservation. No sooner was he informed of the wish to examine the weight and chemical composition of his antique, than he readily sacrificed it to the gratification of this curiosity.

Described.

The obverse of this piece, like that of most of Philip's coins, bears the head of Apollo; and the reverse, a chariot with two horses walking. The name is in the exergue. On similar staters under the legs of the horses appears a monogram, or some type, to denote the mint where the piece was struck. On this stater it is a trident, the symbol of Trœzene.

* Pliny hints, that gold was found in the bowels of the earth sufficiently pure, to be melted without any preparation.

Fourteen

Fourteen staters of Philip are preserved in the rich cabinet of the Florence gallery. Eleven resemble that of Arrezzo on both sides, but they have different mint-marks; one only having the same as that found near Arrezzo. The weight of two of these staters, perfectly resembling each other in external appearance, is precisely 176 Florence grains [133·6 grs Eng.] This is precisely the weight of another stater, the mint-mark of which is formed by a large K, and a small o; of one that has a thunderbolt; one with a vase; and one with an ear of corn, the mark of the Leontini. This being the weight of the six largest staters that have come down to us, there is reason to presume, that it was the weight prescribed for this Greek coin*. Hence it may be inferred, that the drachma was equivalent to 88 Flor. grs. [66·8 grs E.]. (De Romé-de-Lisle gives 4·461 gr. [68·9 grs] for the great attic drachm, that is to say, about 2 grs more.) A proof of the justness of this weight is the attic hemidrachma, or Asiatic drachma, or fourth part of the stater of Philip, which is also preserved in the same gallery, and weighs precisely 44 grs. [33·4 grs E.]. The obverse of this small piece of gold bears the head of Hercules covered with the lion's skin. On the reverse are the bow, vase, and club. The learned and illustrious professor A. L. Millin has sent me the weights of five Philippi in the imperial library; which are as follows. No. 1, 160·5 grs: 2, 161 grs very exactly: 3, 161 grs: 4, 162 grs very exactly: 5, 162 grs. The two heaviest, which differ by an unassignable fraction, are so because they are least worn. The heaviest answers to 175·16 Flor. grs., and is therefore 0·84 of a grain lighter than ours; which therefore may be considered as less worn, and more accurate.

Fourteen in the Florence gallery.

Their weight.

Weight of the drachma.

Attic hemidrachma.

Weights of 5 staters in the imperial library.

Greaves weighed two staters of Alexander, one of which was 133 grs English, the other 133·5. He supposed, that the half grain had been lost by wear; and he concluded, that the drachma should be estimated at 67 grs precisely. The second weight given by Greaves is equivalent to 87·6 Flor. grs. Snellius found the stater of Philip, and of Alexander, to weigh 179 Dutch grs, equivalent to 124·5

Staters weighed by Greaves,

and by Snellius.

* No heavier stater is known to exist.

Eng.;

Eng.*; which, from a comparison with the preceding, would give for the drachma 87·9 Flor. grs; still a little lighter, but very near what we have assigned, or 88 grs.; without its being necessary to estimate the wear, in support of six similar weights in an equal number of gold staters, and with the proof of the fraction mentioned. The celebrated Barthelemi found, from various weighings, that the drachma was precisely 81¼ French grs [66·55 grs E.], which would give about 87·75 Flor. grs. But he would presume a loss of seven eighths of a grain for the wear of 2200 years, and thus gratuitously make the drachma equal to 82 Fr. grs, or 88·5 of ours. It is probable however, that he carries his estimate too high. We should altogether reject from our calculations all allowance for wear; because, by admitting this, we may draw any vague conclusions we please. The weight of 88 grs [66·8 grs E.] is confirmed by a silver drachma of the same Philip, likewise preserved in the Florence cabinet. On the obverse is the head of Hercules, without a beard, and covered with the lion's skin; and on the reverse Jupiter seated, with the eagle on his right hand, and a spear in his left. It is distinguished from others by a lyre and the letter A beneath the seat. The accuracy of the weight of this drachma is confirmed by its half, also in silver, of the same king, which weighs exactly 44 grs. This has the head of Jupiter, ornamented with the diadem; and on the reverse is a figure on horseback, with the name in the exergue, and a mark that cannot be made out. Besides, there are four tetradrachmas of Alexander, of the same metal, the faces and reverses of which are similar; which, weighing all alike 14 den. 16 grs, farther prove the weight of the drachma to be 88 grs. These tetradrachmas are distinguished by various marks, as was said of the staters. One has in the fore part a lamp, and under the seat a moon and a star: another has in the same place the initial T with a circumflex over it, and under the seat the letter E; a third has a buckler, and under the seat a serpent: the fourth has a crown, and under the seat a monogram, composed of an

Weight according to Barthelemi.

Silver drachma of Philip.

Silver hemidrachma.

Tetradrachmas of Alexander.

Mint marks.

* There is evidently some mistake here; but, as I do not know the precise weight of the Dutch grain, I shall leave it as in the original. C.

M barred between the two inner strokes. Lastly we have also a real drachma of this king, of the precise weight of 88 grs, which is distinguished by a monogram, consisting of an H, with a kind of circumflex over the cross stroke. Drachma of Alexander.

Among the tetradrachmas of Thrace in the same cabinet there is one, the twelfth in order, heavier than the rest; and weighing precisely 14 den. 16 grs. This is a proof of the identity of the weight of the Thracians and Macedonians, which had already been conjectured by others*. Thracian tetradrachmas.

After having ascertained the weight of the Philippus found at Arrezzo, it was subjected to cupellation, and the process of parting. Its fineness appeared to be the same as found by Patin; that is 0·979, or 23 carats and a half; containing but half a carat, or 0·021 of silver. The stater assayed.

The art of assaying was known in the remotest times, as the Scriptures attest. In the time of Pliny it had reached such perfection (¹), that the fineness of gold was ascertained from 21 carats, or 0·875, to 21 carats and 7 twenty-fourths, 0·888, and even to 23 carats and 11 thirty-seconds, 0·973. In those days the assay must have been made in the dry way; first by separating the base metals from the gold by means of lead, and afterwards the silver by means of sulphur†, or a sulphuret (⁴). Art of assaying ancient.

The method of refining gold in large quantities was also known, as Strabo says, by cementing or burning it with an argillaceous earth, which, *destroying* the silver, left the gold in a state of purity. Pliny says, that for this purpose the Ancient art of refining. Strabo. Pliny.

* The scholiast on Nicander says, that the didrachma is the fourth part of the Attic ounce: this ounce then must be 704 Flor. grs. [534·4 grs. Eng. Here, as in the other parts of this paper, I have reduced the Flor. grs directly into Eng., agreeably to the values assigned them by Tillet, in the Mem. of the French Academy of Sciences for 1767; paying no regard to the reduction into grammes, made I presume by the French translator, and added in the Ann. de Chim. He gives here 34·496 grammes as equivalent to 704 Fl. grs, which would then be only 532·8 Eng. C.]

† A manuscript written by one Biffoli, who lived in 1460, which is in the Strozzian library, and of which there are several other copies, says: "Parting with aqua fortis was invented about fifty years ago."

gold

gold was placed on the fire in an earthen vessel with three times its weight of salt; and that it was afterward exposed anew to the fire with two parts of salt, and one of *schist*, certainly argillaceous. This would certainly effect the decomposition of the salt, and the volatilization of the muriatic acid in a state of ignition and dry, which would penetrate the substance of the gold, and separate the silver in the form of a volatile muriate; the object (5) and effect of the cementation of the moderns. But Agatharchides has transmitted to us an account of a peculiar method practised in the mines situate between the Nile and the borders of the Red Sea*, in which we perceive the well known property of the muriatic acid in separating silver.

Agatharchides.

This author says, if he express himself accurately, and there be no corruption of the text, that the gold there is enclosed in marble; that the miners burn or calcine this ore: that they break it with hammers, pound, grind, and wash it: and that lastly the gold, placed in a covered crucible with a little lead, some salt, a little tin, and some barley-meal, was exposed to the fire five days.

His description.

The mintmen of Darius certainly employed this or a similar method, when this enlightened king† was desirous of giving his subjects the noble and useful example of money made with the purest gold, similar to that of fine silver made afterward by his satrap Ariander.

Pure gold coins of Darius.

It is not easy, however, to give a plausible explanation of the rationale of the docimastic method transmitted to us by Agatharchides. But if the operation he describes were intended not as a *cementation*, but a real and prolonged fusion, it remains to be explained, how the employment of a closed crucible, kept on the fire as he directs, is to be reconciled with the object proposed: nor is it easy to comprehend the use of barley-meal.

Process of Agatharchides difficult to explain.

But on reflecting on the ingenious method, which Hellot found practised at Lyons, for refining, purifying, and separating cupelled silver from the little lead that remains

A similar method apparently practised at Lyons.

* Gold was extracted from these mines even previous to the discovery of iron.

† The scholiast on Aristophanes ascribes this to another prince of the same name, but more ancient.

with

with it after the first refining, we may form some notion of it (*).

The practice in that city was to take a crucible thirteen inches high, and five inches wide at the mouth: to put a layer of small charcoal three inches deep at the bottom, and cover it with a triangular piece of a crucible, fastened by a little lute at each corner, its sides answering to the corners of the crucible: and on this false bottom to place sixty or sixty-five pounds of silver in long slender ingots, to be melted and purified. The wind-furnace used for this purpose was fourteen inches high, seven in diameter at the grate, and nine at the top. The metal, as it melted, was observed to sink to three inches below the edges of the crucible; and then, when it had acquired a sufficient degree of heat, it was seen to boil like water exposed to a strong fire. In this state it was kept seven or eight hours.

Described by Hellot.

The elastic fluid, which in this case was evolved from the charcoal beneath, caused the agitation here mentioned; the charcoal constituting, as we may say, a kind of bellows ingeniously placed at the bottom of the crucible.

Artificial bellows.

Charcoal, placed in close vessels of glass or metal, we know is not altered, though heated redhot. This we are taught by theory, and the truth is confirmed by many experiments. But the observation reported by the judicious Hellot equally attests, that in this case the charcoal beneath the melted silver is decomposed, and continues to furnish elastic fluid; since this learned chemist found, that silver kept in the same degree of heat, without any charcoal beneath, has a tremulous motion at its surface, and proceeds from the centre to the sides and back again, but does not boil with such noise*: whence then comes the elastic fluid?

Charcoal not decomposed in close vessels of glass or metal: but it is in earthen ones.

Priestley, the founder of modern pneumatic chemistry by an immense number of facts, demonstrates in the most evident manner, what has since been confirmed by many other experiments, that earthen vessels, heated to such a degree as to give a passage to light, are filters, or rather sieves,

This found by Priestley.

* The silver has merely an undulating and circulatory motion.

giving

The process explained on this principle.

giving admission even to the external air*. Thus caloric and light penetrating the bottom of the crucible, and with them the air, attracted chemically by the charcoal within, its oxigen, coming into contact with the incandescent charcoal, inflames a portion of it, combines with it and caloric, and forms carbonic acid. This elastic fluid, through the uninterrupted action of the fire, acquires sufficient force, to overcome the pressure of a column of seven inches of liquid silver above it, and passes through it, agitating it violently. The small residue of lead, which was combined and diffused throughout the mass, being brought by the continual agitation into contact with the carbonic acid gas and the atmosphere (the latter, and perhaps the former, being decomposed by a superior affinity from the concurrence of circumstances), is oxided, and, from the diminution of its specific gravity, is compelled to occupy the upper surface.

The fused oxide of lead rose like an oil.

In fact, Hellot observed a kind of yellowish oil rise from the interior of the melted silver, and float on it. This oil was a pure oxide of lead in fusion; formed by the contact of the continually renewed atmospheric air. The refiners collect this melted oxide, by enveloping and absorbing it with glass or a meagre earth; this earth being removed more readily from the silver it covers, and then the metal remains pure and limpid.

The process of Agatharchides similar.

If we refer to this method the process of Agatharchides, reported above, though very imperfectly, we may suppose, that the barley, or its meal, was employed instead of charcoal, to form what the Lyonese call *the soul of the crucible*; that it was placed at the bottom of the crucible, and retained there by a cover (whence probably the expression of a *closed crucible*); and that on this was poured the gold fused with a little lead, to vitrify the base metals it might contain, and common salt, and sulphuret of antimony or of lead, to seize

* This is denied by many able chemists, who assert, that Priestley was mistaken in his idea; and that the air, in his experiments, was admitted through minute cracks in his vessels, imperceptible to the naked eye. Still this does not invalidate the reasoning of Mr. Fabbroni; for, if this be the true state of the case, air might be admitted to the charcoal in this process through similar cracks in the bottom of the crucible. C.

the

the fine silver, and volatilize it with the lead, or reduce it to scoriæ. The elastic fluids evolved from the vegetable matter by the action of the fire would perform the office of bellows, to agitate the metal violently and incessantly for several days, which would occasion all the impurities to float on the surface, where they would be scummed off as is done by the Lyonese.

But, to say the truth, a fire continued for five days gives rather an idea of the cementation of the moderns, analogous to that transmitted to us by Pliny, than of a real fusion in closed crucibles; a circumstance directly opposite to the purpose intended. Thus in Hungary, the better to open all the interior parts of the gold to the muriatic acid reduced to vapour in the process of cementation, it is customary to add lead to the mass, which is afterward reduced into small hollow drops, or grains as they are called. It is possible, that the lead mentioned by Agatharchides was intended for the same purpose; that tin is a mistaken expression for crude antimony, or native sulphuret of lead; and that the barley meal was intended merely to promote the uniform distribution of the little salt, a stratum of which was to be placed on the gold, and assisted perhaps in decomposing it, as clay or sulphate of iron does now.

Objection.

Hungarian process.

The process of Agatharchides explained by this.

To obtain some light on this curious subject, into a crucible, covered by another inverted over it, were put 720 grs of barley meal, and 576 grs of common salt. This mixture was heated till it acquired the colour of a redhot coal, and in this state it was kept for six and thirty hours. More from curiosity, than to derive any important conclusion from it, into it had been put a small slip of gold, at 21 carats 3 eighths, or 0·891, a third of a millimeter [about 0·13 of a line Eng.] thick, and weighing 24 grs; and a slip of silver, at 11 dwts and half, or 0·958, half a millimeter [near 0·2 of a line] thick, and weighing 40 grs. The lower crucible, in which these were placed, was half full; and in the luting of that above was left an opening of 5 mil. [near 2 lines] for the issue of the elastic vapour.

Experiment to prove its effects.

At the expiration of this time the apparatus, after being cooled, was opened. In it was found a very little earthy residuum, slighly saline, whitish, weighing scarcely 11·5 grs.

Results.

grs. The gold was above it, and increased in weight an eighth of a grain, being perceptibly whitened by the fusion of some very small particles of silver, separated from the remains of the little slip of that metal, which was found sticking upon the gold in the form of an agglutinated dust possessing very little adhesion. These remains were pure silver, and weighed 6 grs and an eighth. The gold, which was silvered only on its surface, was boiled some time in pure nitric acid; when it lost entirely its silvery hue, and was found, on assaying it, to be of 24 carats.

Earthy residuum.

The little earthy residuum was then examined. In it were found no saline particles but a few atoms of muriate of soda, and barely a trace of muriate of copper. The muriate of silver, which from the loss of the metal must have weighed 45·5 grs, had certainly evaporated with the other elastic vapours. In the formation of this muriate only 11·5 grs of muriatic acid had been employed. The 324 grs of acid beside, contained in the salt employed, were dissipated (leaving the small portion of copper out of the question) by a decomposition effected through the means of the vegetable matter mixed with it. But what is difficult to account for, and is foreign to our purpose, is the entire evaporation of 240 grs of soda, which the common salt contained, and which should have remained fixed at the bottom of the crucible. This must have been rendered volatile either by decomposition, or by forming a new compound, and escaped through the opening in the apparatus.

Evaporation of the soda.

Philip used native gold.

It is not probable therefore, that Philip employed similar methods of refining, either by fusion or by cementation, because, I must repeat, he would have reduced the gold to a state of perfect purity, as Darius thought proper to do subsequently; or he would not have confined himself to so small a portion of alloy, or perhaps that alloy would not have been silver. And if he employed the gold as he found it, we must necessarily infer, that nature yields gold at 23 carats and half, or 0·979 (7).

Doubts respecting the fineness of native gold:

Many perhaps will doubt, whether gold be found in nature so near to perfect purity; though Strabo says, that gold was found pure in the Noric Alps; while Pliny is quoted for the assertion, that none is found free from silver. But,

But, without being left in suspense by the assertions and opinions of others, I have the means of removing all doubt on the question; having had an opportunity of ascertaining by my own examination, that gold is actually found native at 24 carats.

but it is sometimes absolutely pure.

I had for some time the keeping of the rich collection of natural history belonging to our first king, who was very fond of these things, and eminently versed in natural philosophy.

Collection at Florence.

In it were many specimens of mineralized gold and native gold, among which I observed two well formed crystals of gold; one cubical, the other a tetraedral prism surmounted by a four-sided pyramid. It would be gratifying to know what substances united to the gold determined these different figures, naturally formed in the bowels of the Earth, and altogether different from those produced in our laboratories by cooling after fusion. The cube is very pale; the prism is of a deeper colour: but these two crystals, which I found by chance in selecting a great many native grains, are unique in the collection, so that it is impossible to think of subjecting them to an examination, that would spoil their figure.

Specimens of native gold in it.

Two singular crystals.

An amorphous but remarkable specimen from Brazil enriched the same collection. It was given by the Prince of Brazil, at Badajoz, to the late king of Etruria, then infant of Spain and hereditary prince of Parma. The weight of this piece is about 14 lbs. [12 lbs, 9 oz. troy]*, beside a small fragment of the same, the nature of which, through the kindness of the king's apothecary, John Ulrici, I was enabled to examine by cupellation and parting; without neglecting to test its solution in nitromuriatic acid by sulphate of iron, and neutral salts with base of potash. By all these trials I was convinced, that it is very pure gold of 24 carats, if the whole mass be homogeneous, without any portion of inferior metal.

Specimen from Brazil,

weighing 12 lbs, 9 oz.

Part of it examined

was perfectly pure.

As no person has ever doubted, that very coarse gold is

* Pliny informs us, that pieces above ten pounds weight were called by the Spaniards in his time *palacras* and *palacranas*; others say, that small pieces were termed *palas*, whence perhaps our *pagliette*, and the French *paillettes*.

found in minerals containing it, I am now certain, that nature likewise presents us with it of the greatest fineness, and even perfectly pure. This is what I purposed to show by this new fact, in writing this little essay, as a present to the lovers of mineralogy and antiquities.

Notes on the preceding Paper by Mr. D'Arcet, Verifier of Assays at the Mint of France.

Pliny's testimony.

(1) Pliny says, book 33, that there is no gold more pure than that obtained from the sands of rivers; and that all gold obtained by arrugiæ has no occasion to be melted, being pure native gold. But Pliny says in the same book, that lead is more malleable and heavier than gold, which is a mistake, and proves, that the gold considered by Pliny as pure was an alloy. He says also farther on, that all gold is mixed with silver; and that the freest from silver known is the gold of Albicrare in Gaul, which contained but a thirty-sixth*: whence it follows, that the testimony of Pliny to this point is of no weight, and that we must appeal to experiment.

Analysis of an ancient coin of Philip.

(2) I delivered to Mr. Mongez the analysis of an ancient coin with the effigies of Philip, which proves, that in his reign coins were made of alloys, the composition of which was native, or at least unknown; for this piece contained

Silver	368
Gold	184
Copper	448
	1000.

It is not probable, that the regulations of the mint required such or so complex an alloy, at a time when the methods of analysis or assay were but approximations; and when they were unquestionably far from the accuracy, that may be obtained even by employing only the touchstone, touchneedles, and prepared acid, used at present.

The art of assaying among the ancients

(3) The art of assaying was as far as possible from perfect in those remote periods. Under the emperors even the fine-

* He speaks of other gold containing a tenth, a ninth, and even an eighth part. C.

ness

ness of gold and of silver was judged by the colour it took in the fire, and that of its streak on the touchstone. very imperfect.

These methods, though practised by experienced men, can give only very inaccurate results, and which may be varied by a number of circumstances; as strong cleaning by aqua fortis, a complication in the alloy, a difference in the alloy, &c.

Archimedes would not have applied the laws of specific gravity to ascertain the falsification of the crown of Hiero, if he could have done it by a better method, and particularly by a method known and commonly practised.

It is well known too, that, under the triumvirate of Mark Antony, every street in Rome erected a statue to Marius Gratianus, who had invented and introduced one of these approximative methods, that have been mentioned; and this denotes the infancy of a useful art, the first steps of which are highly encouraged, because they are considered as conducive to the public welfare.

(4) By employing alkaline sulphurets the solution of gold may be effected: metallic sulphurets only must be understood here. Sulphurets.

(5) Mr. de Robilant, in his account of the processes employed in the mint of Turin, says, that cementation is the process of refining commonly employed at Venice, Genoa, and Florence, where zechins are coined of nearly pure gold. Italian mints.

(6) As Mr. Fabroni says, it is not easy to explain the grounds of the process described by Agatharchides, or of that which appears to be still practised at Lyons. These processes should be repeated, attending to their progress with care, and applying to them the means of modern chemical analysis, particularly the pneumatochemical apparatus. The nature of the gas that traverses the fluid silver should be ascertained, why it forms under such a pressure, why it does not flow back through the pores of the crucible, &c. Process of Agatharchides.

The experiment related by Mr. Fabroni does not appear to me sufficiently conclusive, to decide the question.

(7) Reaumur says, Mémoires de l'Ac. des Sciences, An. 1718, p. 87, that Fineness of gold dust in Europe.

The gold of the river Cèze is at	18 car.	8 grs
Rhone	20	
Rhine	21·25	
Arriège	22·25.	

Lumps of native gold not homogeneous.

He further observes, that the fineness varies in the same piece of native gold. He says, that the piece of 56 marks, which was seen at the Academy, was in one place 23 carats and half, in another 23 carats, and in another 22. The piece of 63 marks belonging to father Feuillée was at its upper part 22 carats 2 grs; a little lower, 21 carats, $\frac{1}{2}$ gr.; and at two inches from the bottom only 17 carats and half. (Reaumur's grain is a twelfth of a carat, a division used in Germany.)

Wicklow gold.

Mr. G. A. Deluc announced in the Journal de Physique, vol. LII, p. 205, that pieces of gold, found in the county of Wicklow, in Ireland, contained a ninth of their weight of silver, without any other alloy.

Piece belonging to the academy.

My father, having been appointed to assay the piece of native gold belonging to the academy, during the time of the revolution, made two assays of it, both of which were 23 car. 26 thirty-seconds. This comes very near to pure gold; and proves, that gold is found in nature alloyed with very variable quantities of silver.

Pure native gold.

Mr. Fabbroni is the first who has demonstrated, that gold is found also quite pure. This is an important observation; but it does not seem to me to overturn the general principle, that native gold is a natural alloy of gold and silver: a principle established by a great number of facts, and to which only one exception is yet known.

Presence of lead in ancient coins to be sought.

It is desirable, that the presence of lead should be sought for in ancient coins or medals: as this would be the most certain method of ascertaining, whether the ancients refined their gold, or employed it as nature gave it them.

VII.

VII.

A Rejoinder to a Paper published in the Philosophical Journal, by Dr. MARCET, *on the Animal Fluids. By* GEORGE PEARSON, *M D. F. R. S., &c.*

To W. NICHOLSON, Esq.

SIR,

BY a severe accident I have been prevented from writing the paper, which I proposed in the communication honourably inserted in your Journal for February last. Meanwhile an answer has been published by Dr. Marcet*.

Before I redeem my pledge of offering some remarks on Dr. Marcet's Memoir, the subject of my former communication, I feel myself called upon by what I consider to be the true interests of science, to reply to his intervening answer. This gentleman cannot be more averse from polemical writing than I am, nor have more powerful motives of private advantage by being otherwise employed: but unless I were to avail myself of the plea of a celebrated philosopher, who asserted, that his regard for *truth* was so great, that he would not part with it, lest it should be ill treated by mankind, I have no option consistent with public duty. The feelings of either party must however regulate their future conduct. For myself I can only promise, that I shall not consider it as a point of honour to contend for the last word.

Reply to Dr. Marcet.

In the answer, which has been addressed to me, Dr. Marcet has set forth evidence from his memoir, still under examination, to maintain, that soda in an uncombined state, and not potash, exists in the animal fluids, as I trust I have legitimately proved according to facts hitherto discovered. As my honourable Opponent has not contravened the most decisive parts of the evidence in support of my allegations, I am spared the pains of again displaying it; so that I have only to comment on the evidence he brings forward in justi-

Whether the animal fluids contain soda, or potash, uncombined.

* See the Philos. Journal for March last.

fication.

fication. In my remarks perhaps I cannot entirely avoid repetition of objections already produced.

Figure of crystals not a decisive proof.

The first kind of proof, that soda and not potash is present, again asserted by my adversary, is from the figure of crystals. I have to remark in addition to my former observations, that their forms *alone*, rarely or never, even when perceivable with the unassisted organ of vision, do singly denote unequivocal properties: and when not perceivable without the medium of glasses, we know from past experience the figures are to be considered as still more equivocal, I might say deceptive. If these crystalline forms are now admitted as justly distinguishing properties of certain substances, it is in consequence of repeated observation on larger quantities by direct vision, "quæ sint oculis subjecta fidelibus"; but even then not without concomitant other well ascertained properties.

Acetic acid said to have formed acetate of soda.

Secondly, great dependence seems to be placed on the acetate produced by combining acetic acid with the saline matter afforded by incineration. This was said to be acetate of soda, which dissolved in alcohol, "while potash was found in the residue left undissolved by the alcohol". I have searched the pages of the memoir under examination, again and again, for the evidence in support of this allegation; but, here and on many other occasions, is a mere assertion, except a partial support from the serum of the blood, as will be seen hereafter. For 1st, with regard to the saline matter of the fluid of the *spina bifida*, I find these words, "the alcoholic solution being decanted off and evaporated to dryness, a residue *supposed to consist of acetate of soda* was obtained." Here no mention is made either of an experiment to prove whether the acetate was that of soda or of potash, but it was *supposed* to be acetate of soda. As to the undissolved matter containing potash, there is not even that I can find a word written. This too, has been *supposed*.

Fluid of spina bifida.

Fluid of hydrocephalus internus.

2. With regard to the second fluid examined, that of hydrocephalus internus, we are told, "the analysis was conducted in the same manner as in the former"—of course the existence of soda in the alcohol, and of potash undissolved, is not proved, but here also *supposed*.*

3. In

3. In the examination of other animal fluids, viz. of ascites, of hydrothorax, and hydrops pericardii, as well as subsequently of the hydrocele, of the hydatids, of the thyroid gland, and of a tumour of the chest, no such experiment as that of compounding an acetate is mentioned. Other fluids.

4. In the experiments however on the saline matter of the serum of the blood, an acetate was compounded, which dissolved in alcohol; the words of the author being, "the alcoholic residue, *contrary to my expectations*, exhibited traces of potash, both by means of tartaric acid, and oximuriate of platina." This, as far as I can find, is the sole experiment with acetic acid and alcohol, related by the author to determine the kind of alkali present, although the assertion is made of the animal fluids generally. But, although the assertion be not proved, it may be worth while to consider what, or whether any thing is proved by these experiments. They prove, that potash was present, because there was a precipitate with tartaric acid, but nothing more—there is no proof, that it was in the state of muriate, as asserted. It perhaps will be said, that these experiments prove, that this "alcoholic residue" contains also acetate of soda; "for the same residue, treated with nitric acid, was almost entirely resolved into rhomboidal crystals, among which I was unable to detect any distinct prisms." Now I have already expressed my want of confidence in the figure of minute crystals *singly* as evidence, especially seen through glasses; and here I presume is a decisive instance of their fallacy; for the potash being proved to be present, and, as already said by Dr. Marcet, united to muriatic acid, it must have afforded cubes, if reliance can be placed on forms; but no such cubes were seen. A farther objection occurs to my mind in this experiment. I apprehend, that it is quite as likely to be true, that alcohol will dissolve a small proportion of muriate of soda, as according to Dr. Marcet it does of muriate of potash. This being the case, the "alcoholic residue" ought to have afforded cubes of muriate of soda as well as of muriate of potash. The process under examination requires farther animadversion on the remaining part of it: "Potash was easily discoverable in the residue insoluble in alcohol, which residue had now lost its deliquescent

Serum of blood. Experiment with acetic acid and alcohol.

What is proved by it?

deliquescent quality." That potash was present in a combined state I admit may be inferred, but I say confidently there is no proof, that it was united to muriatic acid. It is not however incumbent on me, but on the Affirmer, to show with what it is combined. I think it right to notice another unsatisfactory part of the process before me. It is said, a concentrated solution of the saline mass in question did not distinctly indicate potash by oximuriate of platina, but did by tartaric acid. Subsequently however we are told, that the dissoluble, as well as the indissoluble residue, of the acetous compound in alcohol readily denoted the presence of potash to the oximuriate of platina as well as to tartaric acid. To me I own this account only shows, that the quantities employed were too minute for distinct observation of facts. How all ambiguity might have been removed I have taken the liberty of proposing in commenting on this process in my former communication to your Journal, p. 151. On that occasion I expressed my doubt, whether or not the acetate of soda be dissoluble in alcohol, but I referred to the authority of experiment. Here, my learned friend exultingly construes these phrases of doubt, *two palpable errours*, and triumphs—" a hit, a hit, my Lord, a very palpable hit."—No, there is no errour in this case, Dr. M., according to the *English* meaning of the terms used. To make the most of these *asserted errours* I am also charged with no less than three times repeating them; as if the propriety of writing was absolutely limited to the number of times an assertion should be delivered. At this time however, without the slightest uneasy emotion, I say, that acetate of soda is a deliquescent salt, and dissoluble in alcohol; for I have performed the necessary experiment, not indeed with " half a grain and a watch glass," but with 50 grains. The truth is, I had not leisure, little as was required, when I wrote my communication, to make the experiment; but as, on inquiry of a friend most likely to be informed, I found he was ignorant; as on just looking into two valuable books, Aikin's Dictionary and Thompson's Elementary work, one said the acetate of soda was a permanent and the other a deliquescent salt; and as in my collection of specimens, there was a permanent crystallized salt

Another point not satisfactory.

Acetate of soda deliquescent, and soluble in alcohol.

salt labelled by an assistant acetate of soda; I thought it best to leave the matter as doubtful, although I own I inclined to the contrary opinion of that which is I now believe the truth. Dr. M. may call this a *palpable errour*, if he pleases—he will hurt nobody but himself by the phrase. The main proof is hereby not affected; for the fact now ascertained against my doubtful opinion is only a collateral evidence on either side.

5. Another source of evidence against me is that potash combined "was proved by the tests oximuriatic of platina, and tartaric acid." The just inference has been already proposed; but I will now remark that this experiment does not prove, that soda was or was not present. **Proof of combined potash none of soda.**

As to any other proofs they have been already minutely examined in my former communication, or have been answered in this: but I entreat the indulgence of being allowed to make two or three farther remarks. 1. On the fluid of the spina bifida, of the thorax, and of the pericardium, the tartaric acid was not employed at all. Of these fluids the analysis in general was very partial. 2. Of the alkaline matter of the hydrocephalus fluid the examination must be unsatisfactory by the tests, on account of the impracticability of entirely separating the two alkalies from one another in such minute quantities as were obtained; and, if the separation were not effected, as the two fixed alkalies are affirmed to exist, the test, tartaric acid, must have produced soda-tartrate of potash; consequently the inference of the adverse party cannot be just. **Other insufficient evidence.**

Having, as briefly as seemed proper, commented on the opposing evidence, and set forth in a different light my own, I must pay due respect to the other parts of the ingenious Answerer's paper.

If it shall appear, that the only difference in the results of the inquiries by the two parties, worth particular notice, is with respect to the alkaline matter, I submit to the world, whether or not Dr. M. could with prudence have published his memoir without a reference to his predecessor, as he observes he could have done with propriety; and especially as he says he was directed particularly to the alkaline impregnation by my paper. Dr. M. complains, that he is **Farther remarks.**

at

at a loss to understand my meaning, and is much embarrassed by my obscure and inaccurate manner of writing. I am grieved, that my learned friend should experience these difficulties; but as I have not heard similar complaints from others, I may perhaps not indecorously venture to say, that I suspect his claim to judgment of propriety and perspicuity in English is somewhat doubtful.

Whether substances be more distinguishable in large bodies, or in small.

My ingenious Opponent cannot agree with me, that substances and properties of substances are discoverable by operating upon large masses, which cannot be effected with smaller quantities. I really thought the proposition so obviously true, that illustration was needless. Heaps of illustrative examples in nature occur to my mind, while I am writing, both in the department of chemistry and physiology. If arsenous acid, muriate of soda, or sulphuric acid, be dissolved in the proportion of one part to 100 equal parts of water, they will be discoverable by well known reagents; but if the proportion of water be increased more and more, the indication of their presence will become less and less distinct, and at last they will be no longer perceivable, although it is known they exist: or if I take certain fractional designated parts of any given weight of these substances, they will elude manifestation by any means hitherto known. On this principle of division and diffusion the most deleterious poisons become innoxious by the minuteness of the quantity applied to the human constitution. Hence atmospheric air containing fen miasmata, plague contagion, or small pox matter, are applied with impunity. A pound of blood of a glandered horse transfused into a healthful horse will not excite disease, but as much blood as can be transfused from two glandered horses into one horse will excite the disease of glanders. Sugar, alkali*, &c. may exist in the blood, but not be discoverable by any known reagent on account of the small proportion of them existing in the blood at any given time, as I humbly reason,

* In Dr. Rollo's work on diabetes I have related an experiment, in which potash was taken in such quantity, that the urine became so impregnated as to afford a precipitate of super-tartrate with tartaric acid, at the same time the blood did not indicate a trace of alkali; owing, as I concluded, to the small proportion of alkali to the blood.

and

and not on account of an hypothetical new channel, a sort of northwest passage from the stomach to the urinary bladder. In the case of waters the proportion is so minute of various impregnating substances, that, unless very large bulks be used, they must escape detection. The great masters have accordingly used such large quantities. Margraf, (Opuscules chymiques, v. II, p. 8) did not evaporate 100 drops of snow or rain water in a watch glass capsule like some modern microscopic chemists, but he operated upon 100 quart measures of snow water, in which he was able to find only 60 grains of carbonate of lime, a few grains of muriate of soda, and traces of nitrous acid.

I had the advantage of making my juvenile efforts to perform several chemical exercises under that great master, professor Black. Among other precepts, treasured in the tablet of my memory for more than 30 years, was that of employing large bulks of mineral waters; and of all other things, in which there was a probability of minute proportions being present. The reasons of Dr. Black for not practising according to this rule in the instance of the analysis mentioned I cannot pretend to assign; but it seems probable, that he was in possession of only a small quantity of the material. As to the magnitude of the masses of matter required, it is impossible to specify them; but it is obvious, that analysis must fail to develope substances on account of the minute proportion to other things with which mixed not being susceptible of being made evident to the senses; and in consequence, by a due larger proportion they may be rendered sensible. Hence perhaps, it is that we are ignorant of many of the properties of light, calorific, electricity, of infectious, and contagious substances, &c. It is argued against me, that "the chemical properties, which belong to a particle of matter, belong to the whole mountain of the same substance." True—but I know nothing of properties of substances but by means of the external organs of sense, (this is indeed an axiom) and unless the particle be of a due magnitude, my organs of sense cannot inform me of its properties.

My honourable adversary talks of the advantage of a small scale of operation in the points of economy and convenience. Granted— Advantages of a small scale of operations

denied in some respects,

Granted—but these are minor considerations indeed to the acquirement of knowledge. When Dr. Marcet also speaks of the advantage in point of accuracy, I protest against it for reasons above explained. It is farther represented, that "there is a degree of *neatness* gained by reducing the scale of operations". I own I have difficulty to conceive a just sense in which this term may be used on this occasion. Does it mean the avoiding extraneous things occurring in operations? if so, I cannot separate it from *accuracy*; and as it is seldom practicable to operate without meeting with some extraneous matter or dirt, it appears to me, that many of those old chemists, who are reproached for mentioning "a little dirt in their results", are more accurate than those modern chemists, who make up a "neat" tabular exhibition of the constituents of substances in centesimal quantities, which they have never weighed; and even of which substances there is a palpable deficiency of proof. If by *neatness* be meant the instruments employed, it would be as injudicious to prefer neatness to knowledge, as euphony of style to perspicuity.

but admitted others.

A proud list is displayed of discoveries achieved by microscopic experiments, or on small masses of matter; but that was needless. I never disallowed the utility of such experiments. My plain answer is this—that for certain purposes all the knowledge that is wanted is attainable, and most easily, by operations on the small scale—that such is the nature of our present instruments, that it is only practicable to work on small quantities of some kinds of matter—that on almost all occasions it is advantageous to commence an intended perfect investigation with experiments on small masses, in order to enable the mind to invent subsequent experiments, and perform decisive operations on larger quantities. As to the successful practices referred to, they only manifest, that much may be accomplished with inferior means; but it is demonstrable, that the same persons could have attained infinitely more by superior instruments, and in the more favourable circumstances of adequate quantities. In chemistry, I consider illustration by examples to be superfluous. Physic furnishes new illustrations analogous to the questions under discussion. Sydenham, with-

Superiority of experiments on a large scale.

out

out chemistry, with seemingly little of anatomical and physiological knowledge, as well as of natural history, has meritedly acquired the credit of one of the greatest Improvers; if he could acquire so much without these auxiliaries, it appears according to all reason, that by means of them much more would have been achieved. I might however exemplify the advantages for which I am contending by the conduct of Dr. Marcet himself. It appears, that he performed the analysis of two animal fluids, of the component ingredients of which he has given an account to the one hundredth part of a grain, without finding potash in any state. Subsequently however this alkali was detected in other animal fluids, the author's attention being directed, as he is pleased to say, by my published paper on expectorated matter, and by my conversations. Whether otherwise Dr. Marcet would have found out the potash, let others determine. Notwithstanding the sneering remark of his ounce or two of dropsical liquid being in competition with my two or three pounds of "ropy sputum," I should be very unreasonable if I were not, after this practical proof of the inadequacy of his method, to be well contented. If however instead of treading the primrose path of the new microscopic chemical school, he had condescended and submitted to the task of labouring in the "large, dismal, subterraneous laboratory;" if, I say, he had been there employed instead of in dalliance at "the fireside of his comfortable study;" I am confident it did not require his talents, to have done much more than nearly confirm the results of my experiments on animal substances. If too I can see the future in the instant, it will be only by experiments on very large quantities of the animal fluids, that discoveries will be effected of more of their impregnating ingredients, on account of the very minute proportions in which they exist.

Why Dr. Wollaston was mentioned as concerned in the inquiry.

Dr. Marcet thinks it worth while to disclaim his memoir as the joint work of Dr. Wollaston and himself. I cannot have the smallest objection, indeed by this I gain strength to my side; for the demand of justice alone compelled me to consider this writing as I have done. I must however cite a passage for justification. Beside the advantages from Dr. Wollaston's writings and conversations Dr. Marcet

Marcet owns "his *kind personal assistance in this and other similar inquiries*."

Charge of misquotation,

I am accused of the unwarrantable licence of "quoting in italics, and placing between inverted commas words which have not been used by my adverse friend." Such base proceedings I am charged withal! As for italics I knew no better than that all writers for the sake of emphasis do employ such letters either for their own words or those of other writers. The word *elegant* so complained of is not intended as a quotation, it is my own word, which Dr. M. misrepresents. As for inverted commas, the few passages which they include I think no one would apprehend are Dr. Marcet's writing, except in two or three instances. Here I cannot perceive any misquotation but in one place. There I confess my heinous offence, and express sincere contrition, viz. for "fireside of the *drawing room*," in future read "the large, dismal, subterraneous laboratory is now changed for the fireside of *a comfortable study*."

of irony,

Again; my respectable adversary is, I find offended, with what he is pleased to construe *irony*. I can do no more than declare, whether I shall again be accused of irony, or not, that I entertained more of respect than sufficient for subduing any such humour.

of jocularity.

Another offence is *jocularity*, not suitable for the advancement of science. If in such a vein I have offendingly written, "I have shot mine arrow o'er the house and hurt a brother." This mode of writing however has the high authority of a great poet, and still greater philosopher—

> .... ridentem dicere verum
> Quid vetat?

I wish I could more frequently be jocular, as so many occurrences are experienced in common life to make one sad. Hence I would rather live with Horace, than with the melancholy moralist Jaquez. Some allowance too should be made for the differing natures of individuals from the elements being so differently mixed up.

> "Nature hath fram'd strange fellows in her time,
> Some being of such vinegar aspect,
> That they'll not show their teeth in way of smile,
> Though Nestor swear the jest be laughable."

The

The foregoing pages of rejoinder will, I trust, save me the trouble of many intended remarks on Dr. Marcet's paper, independently of its relation to the questions at issue. A few comments only I shall now beg to be allowed to deliver.

Substances found in animal fluids.

1. The *animal matters* in the fluids examined are stated to be of *two kinds*: viz. coagulable or albuminous matter, and what the author calls *muco-extractive*. I do not at all object to the experiments, but appeal to competent judges, whether it is not unjust to make this distinction. The evidence of the *coagulable* matter is from the visible coagulation by calorific, and some reagents, but if there be not a due proportion of it to the water in which it is dissolved, such evidence is not obtainable. This may be easily proved, and as I apprehend I have shown in my published papers, by a kind of synthetic experiment. For example: serum of blood, or any other known coagulable fluid, may be so diluted with water, as to afford no clear proof of its presence by coagulation on applying calorific, although such an effect may be reasonably inferred on probable grounds from the disturbance of transparency, or cloudiness. And, as far as I have found by experiment, coagulable matter so diffused, on being collected by evaporation to dryness, is scarcely coagulable by calorific; so that the whole of any given quantity of animal coagulable fluid by such treatment was rendered uncoagulable. According to my trials too, there always remained, on coagulating serum and other analogous fluids, a small proportion of animal matter dissolved in the watery part, which differed in no respect from the matter left on evaporating water containing a certain small and uncoagulable proportion of serum added to the water as above stated. But these dilute solutions, which appear uncoagulable, denote the presence of animal matter to the test of tannin. It was probably this property, and the animal matter afforded by evaporation, which induced some chemists to conclude, that a different kind of animal substance from coagulable, such as gelatinable, existed in the serum of blood. Hence I conclude, that the two grains of what Dr. Marcet calls *muco-extractive matter*, afforded by 500 grains of serum, after separating 44 grains of albumen or coagulable matter, is this matter rendered uncoagulable by dissolution. And hence too I conclude, that the animal

The animal matter in the fluids of one kind only.

animal matter, in the other animal fluids, which he examined, was of *one* kind only, viz. coagulable matter, but not demonstrable by its most distinguishing property on account of dissolution in a large proportion of water.

Ammonia not mentioned.

2. *Ammonia* is not mentioned among the impregnating ingredients. This is to me not surprising, for it is evidently from my experiments in so small a proportion as to be undiscoverable in the quantities employed. If I could not find by estimation half a grain weight of it in 7 or 8000 grains of animal matter, it was not likely to be rendered evident in 7 or 800 grains.

Sulphate of potash.

3. *Sulphate of potash.* That a *sulphate* exists I perceived evidence, and have accordingly inserted it among the saline matters in my published papers; but that it is *sulphate of potash* I apprehend will not be allowed to have been shown by Dr. Marcet.

Phosphate of lime, iron, and magnesia.

4. *Phosphates of lime, of iron, and of magnesia,* are enumerated in the memoir before me. Of *phosphate of lime* there is good evidence, as I have set forth, and coincide in my results with those of the author: as well as that there is probably sulphate of magnesia: also, that there is iron; but I was not able to infer, that it was in a state of phosphate, I only inserted it in my results as an oxide.

The colouring matter of the blood not iron.

Although it is not essentially connected, I take this opportunity of referring to a process, which I offer as evidence against the common opinion, that the colour of the blood is owing to iron. I have mentioned it in my lectures during several past years, and it was published in the Edinburgh Medical and Surgical Journal, vol. VII, p. 124, for January, 1811. I collected 110 grains of the red part in a dried state, by repeated ablutions from about 10000 grains, or upwards of twenty ounces of blood. By burning in a platina crucible, it afforded, in weight, two grains and a half of a half-fused brown tasteless substance. By boiling in muriatic acid a part was dissolved. This solution was not styptic to the taste; it became blackish on adding tincture of gall nut, and on adding prussiate of potash it afforded a deep blue coloured precipitate, which did not yield by ignition, or calcination, above half a grain of reddish brown powder. Is it then probable, that twenty ounces of blood should derive their colour from half a grain

of

of oxide of iron? I think proper to speak of this result at this time because it was published anonymously, and because subsequently to its publication I find it has been mentioned by other persons without acknowledgement, or at least without knowledge of this fact.

5. I found also indications of *caronate of lime* and of *silica*, not enumerated by Dr. Marcet. Future experiments must furnish unequivocal evidence. Silex, and carbonate of lime.

6. *Muriate of potash* asserted by the author, instead of *potash* united to animal matter, or to some other destructible substance, as I have inferred. On this question perhaps more than necessary has been already said in the present and former papers. Muriate of potash.

7. *Subcarbonate of soda*, asserted by the author, has been the subject of discussion at the same time as the last mentioned ingredient. Subcarbonate of soda.

8. *Muriate of soda*. Both parties agree in this being the chief saline impregnation. Muriate of soda.

It may be right to notice, that I have employed the term *self-coagulable lymph*, instead of the usual one *coagulable lymph*; because the serum, another fluid of the blood, is also *coagulable*, but not of itself without a certain temperature, or certain substances being mixed with it. The deposit spoken of by Dr. Marcet is not, I think, as he supposes, what I mean by the term self-coagulable lymph. Self-coagulable lymph.

Although, if the cause of truth require it, another communication may be offered; it will be most agreeable to me, that it be not found necessary. Considering the erroneous inferences, with which the writings of chemistry by men of the greatest celebrity abound; I shall on that account endeavour to find a source of consolation, if time show, that I am the erring party. I hope too, that this controversial discussion may serve to promulgate knowledge, by inducing some persons to attend to the subject, who might not otherwise have known the original papers. If with these reflections my respectable adversary can be satisfied, the controversy will now be terminated. Conclusion.

"Claudite jam rivos pueri: sat prata biberunt".

George Street, Hanover Square, G. P.
April the 17th, 1812.

VIII.

METEOROLOGICAL JOURNAL.

1812.	Wind	Pressure. Max.	Min.	Med.	Temperature. Max.	Min.	Med.	Evap.	Rain	
3d Mo.										
March 6	S W	29·88	29·84	29·860	56	41	48·5	—		☽
7	N W	29·87	29·66	29·765	57	35	46·0	·12	0·18	
8	N W	30·19	29·87	30·030	50	31	40·5	—	3	
9	N E	30·26	30·19	30·225	46	33	39·5	—		
10	N E	30·26	30·20	30·230	44	30	37·0	—		
11	N E	30·20	30·20	30·200	46	33	39·5	—		
12	N W	30·20	29·96	30·080	44	34	39·0	—	0·12	
13	N E	29·99	29·96	29·975	45	34	39·5	—	6	●
14	N E	29·99	29·87	29·930	44	26	35·0	—	4	
15	N E	29·87	29·76	29·815	42	31	36·5	·48	1	
16	N E	29·77	29·75	29·760	35	31	33·0	—		
17	N E	29·75	29·66	29·705	36	29	32·5	—		
18	N E	29·66	29·40	29·530	39	26	32·5	—		
19	E	29·40	29·30	29·350	39	29	34·0	—	0·14	☾
20	S W	29·24	29·10	29·170	50	40	45·0	—	8	
21	S E	29·54	29·24	29·390	54	39	46·5	·30	0·18	
22	N E	29·74	29·54	29·640	53	39	46·0	—	2	
23	S E	29·74	29·27	29·505	42	40	41·0	—	0·67	
24	N W	29·64	29·27	29·455	40	32	36·0	—	0·16	
25	N E	30.27	29·64	29·955	42	24	33·0	—	1	
26	S E	30·35	30·17	30·260	46	30	38·0	—		
27	S E	30·20	29·46	29·830	51	41	46·0	·36	0·16	○
28	S	29·42	29·25	29·335	53	49	51·0	—	—	
29	S W	29·48	29·36	29·420	58	48	53·0	—	0·46	
30	S W	29·78	29·48	29·630	59	40	49·5	·30	0·12	
31	E	29·53	29·48	29·505	47	40	43·5	—	0·10	
4th Mo.										
April 1	Var.	29·64	29·59	29·615	58	41	49·5	·18	—	
2		29·70	29·58	29·640	—	—	—	—	—	
3		29·68	29·58	29·630	55	43	49·0	·17	0·26	
		30·35	29·10	29·739	59	24	41·5	1·91	2·80	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES.

NOTES.

Third Month.—9. A shower of hail p. m. 11. Hoar frost. 15. Frosty morning. 16. Wind very strong from N. E. all day. 17. Cold wind. 20. Snow in the morning, followed by rain. 22. Very wet night: high wind. 25. Snow: the barometer rising rapidly. 26. a. m. Very fine: barometer still rising. 27. a. m. Cloudy; a considerable depression of the barometer, with appearances indicating thunder. Late at night a shower of hail, with lightning. 28. Stormy, with showers. 29. a. m. Windy. At 2 h. 30 m. p. m., the temperature without being 54°, I found the vapour point in a room as high as 51°. In an hour after this it began to rain steadily, and there fell near half an inch depth. 30. Much wind, at intervals changing to E. 31. Stormy from E. and S. E.: cloudy: about 9. p. m. an extensive appearance of light in the clouds to the W. with rapid coruscations passing through them, in the manner of an aurora borealis. This phenomenon was apparently not more elevated than the clouds which then overspread the sky, and was certainly not produced by the reflection of a light situate below them: it continued 20 or 30 minutes.

RESULTS.

Prevailing winds easterly.

Barometer: highest observation 30·35 inches; lowest 29·10 inches; Mean of the period 29·739 inches.

Thermometer: highest observation 59°; lowest 24°; Mean of the period 41·5°.

Evaporation 1·91 inches. Rain, &c. 2·80 inches.

This, as well as the preceding lunar period, has been unusually productive of rain: the two afforded *six inches and a half* in fifty-nine days.

LONDON,
Fourth Month, 22, 1812.

L. HOWARD.

IX.

IX.

A chemical Account of an Aluminous Chalybeate Spring in the Isle of Wight. By ALEXANDER MARCET, *M. D., F. R. S., one of the Physicians to Guy's Hospital, and Member of the Geological Society*.*

Analysis of mineral waters.

THE accurate analysis of a Mineral Water, although attended with considerable difficulty and labour, must be allowed, in a general point of view, to be an object of so little importance, that unless there be some interesting medical question to investigate, or some new analytical methods to point out in the course of the inquiry, it may be questioned whether researches of this kind are worth the time and attention which they require, or deserve to be placed amongst the records of natural science.

Importance of the present subject.

Having thought it necessary, in the present essay, to confine myself to the natural and chemical history of the spring in question, without any digression upon its medicinal qualities, and being well aware, that chemical details are considered by geologists merely as collateral subjects, some apology may be required for the length of this communication. But if the relation which the history of mineral waters bears to geological and mineralogical inquiries, and the peculiarities of composition, for which this spring is remarkable, entitle the subject to the attention of this society, I hope, that the general views and investigations which I have occasionally introduced respecting the analysis of mineral waters, and the composition of several salts connected with this inquiry, will be deemed a sufficient excuse for having thus expanded an account, from which they were almost inseparable.

Inducements to the analysis.

It is about two years since my attention was directed to this chalybeate spring by Dr. Saunders, to whom, in consequence of his valuable treatise on mineral waters, inquiries of this kind are frequently referred. Having been requested by him, and soon afterward by the discoverer of the spring, Mr. Waterworth, surgeon, of Newport, to examiue this water, I

* Transactions of the Geological Society, vol. I, p 215.

soon

soon perceived by a few preliminary experiments, that its principal ingredients were sulphate of iron and sulphate of alumine, and that it possessed a degree of strength far more considerable than any mineral water of the same kind that ever came to my knowledge.

This last circumstance, and the probability that this spring might some day attract public notice from its medicinal properties, induced me to undertake the present analysis, which, after many interruptions, I have at length brought to a conclusion.

SECT. I. *Situation and Natural History of the Spring.*

This spring is situate on the south-west coast of the Isle of Wight, about two miles to the westward of Niton*, in one of those romantic spots for which that coast is so remarkable. Situation and natural history of the spring.

In its present state it may be said to be of difficult access, for there is no carriage road, nor even any regular foot path along the cliff leading to it, and the walk would appear somewhat arduous to those unaccustomed to pedestrian excursions. But it would be practicable, and probably not very expensive, to render this path equally easy and agreeable. It was in walking along the shore, a few years ago, that Mr. Waterworth's attention was accidentally directed to this spring, which he traced to its present source, by observing black stains formed by rivulets flowing from that spot.

With regard to the mineralogical history of that district, I have been favoured through the kindness of my friend Dr. Berger, who visited the spot very lately, with so much more accurate an account of it than I should, from my own observation, have been able to offer, that I shall make no apology for transcribing it in his own words. Mineralogy of the district.

"The aluminous chalybeate spring", says Dr. Berger, "issues from the cliff on the S. S. W. coast of the Isle of "Wight, below St. Catharine's seamark, in the parish "of Chale. The bearing of the Needles from the spot is

* On an Estate belonging to Michael Hoy, Esq.

"N. W.

Situation and natural history of the spring.

" N. W. while that of Rockenend, not far distant, is S. E. " by S.

" The elevation of this spot, as far as I could ascertain " it by the barometer, is one hundred and thirty feet above " the level of the sea. Its distance from the shore may be " about one hundred and fifty yards.

" The water is received into a basin formed in the rock " for this purpose, and flows, as I was informed, at the " rate of two or three hogsheads in a day. Its temperature " I found to be 51°, that of the atmosphere being 48°; " and it may be worth while to observe, that this tempera- " ture corresponds with that of several springs of pure wa- " ter which I have met with in the island.

" The lower part of the cliff is rather encumbered with " masses of rock, or portions of soil, which have fallen " from the upper strata. Immediately above these, the " spring issues from a bed of loose quartzose sandstone " containing oxide of iron. This sand, in which vestiges " of vegetable matter are discoverable*, alternates with a " purplish argillaceous slate of a fine grain, disposed in " thin layers, with a few specks of silvery mica, interspersed " through the mass, Black stains or impressions of vege- " tables are seen on the natural joints of this rock. Above " this lies a stratum, several fathoms in thickness, of a " blueish calcareous marl, with specks of mica, which has " an earthy and friable texture, and contains imbedded " nodules or kidneys of sulphuret of iron. Many of these " nodules have undergone a partial decomposition, to which, " no doubt, the existence of the principal ingredients of " the spring is to be ascribed. The upper strata of the " cliff are composed of a calcareous freestone, alternating " with a coarse shelly limestone, accompanied by nodules " or layers of *chert* or flint.

* On being sprinkled on a heated shovel, this sand scintillates as if undergoing a partial combustion. When submitted to chemical analysis, it yields a quantity of iron, but no lime, nor alumine, nor any other earthy matter soluble in an acid. Close to the spring this sand contains some traces of sulphuric acid, but not at a distance from it: it is evident therefore, that the sand rock is not the medium through which the spring is impregnated.

" As

" As the same arrangement of rocks here observed pre- " vails in several other parts of the Isle of Wight, and even " along the coast of Hampshire, it is not improbable, that " other springs of a similar nature might be discovered. " May not *Alum Bay*, which lies to the north of the Nee- " dles, have derived its name from a circumstance of this " kind?

Perhaps similar springs in the neighbourhood.

" On the road from Shorwell to Chale, the soil consists of " a ferruginous sandstone, and chalybeate iridescent waters " are to be seen in several places. To the east of Fresh- " water bay, not far from the place where the cliffs of chalk " begin to make their appearance, there is a rivulet, the taste " of which strongly indicates the presence of iron. At " Blackgang Chine, a little to the N. W. of the aluminous " chalybeate, is another ferruginous stream running to " the sea. The rock there is a sort of decomposed iron- " stone under the form of balls. The sound compact " ironstone, having the appearance of flat pebbles worn " by the rolling of the sea, occurs not unfrequently along " the shore.

Other chalybeate waters.

SECT. II. *General Qualities and specific Gravity of the Water.*

a. The water issues from the sand rock above described perfectly transparent, and it continues so for any length of time, provided it be collected immediately, and preserved in perfectly closed vessels; but if allowed to remain in contact with the air, or even if corked up after a temporary exposure to it, reddish flakes are soon deposited, which partly subside, and partly adhere to the inside of the vessel.

General qualities of the water.

b. It has no smell, except that which is common to all chalybeates, and this it possesses but in a very slight degree.

c. Its taste is intensely chalybeate, and, beside a considerable degree of astringency and harshness, it has the peculiar kind of sweetness, which sulphate of iron and sulphate of alumine are known to possess.

d. Its specific gravity somewhat varies in different specimens. In three different trials I obtained the following results:

1st

1st specimen	1008·3
2d specimen	1007·2
3d specimen	1006·9
	3022·4
which gives a mean specific gravity of	1007·5

SECT. III. *Preliminary Experiments on the effects of Reagents.*

Experiments on it with various tests.

A. Paper stained with litmus was distinctly reddened by the water.

B. Paper stained with Brazil-wood was changed to a deep purple.

C. When agitated in contact with the air, or repeatedly poured from one vessel into another, the water became turbid, and on standing deposited reddish flakes.

D. On applying heat to a portion of the water just uncorked, and boiling it *quickly*, till it was reduced to one half or even one third of its original bulk, no precipitation whatever took place; but on continuing the evaporation, a white feathery crystalline substance appeared on the surface of the fluid, and on pushing the process still further, a saline matter of a pale yellowish green colour appeared, which continued to increase till the whole was reduced to a dry yellowish mass. These were the phenomena observed with water recently uncorked; but when, previous to the evaporation, it had been for some time exposed to the air, or when the evaporation was conducted very slowly, an appearance of reddish flakes was the first circumstance observed.

E. The mineral acids produced no obvious change in the water.

F. Oxalic acid produced a slight yellowish tinge; but no immediate precipitation or turbidness.

G. Oxalate of ammonia, in small quantity, likewise produced a yellow colour, without precipitate: but on adding more of this test a white precipitate appeared.

H. Prussiate of potash and infusion of galls produced abundant precipitates, the one blue, and the other black or dark purple; and the colour of these precipitates was much

paler

paler when the water had not previously been exposed to the atmosphere.

I. Alkaline solutions produced copious greenish floccu-lent precipitates, which became darker on standing in the air.

K. Nitrate of silver occasioned a dense, white, but not considerable precipitate.

L. Both muriate and nitrate of barytes occasioned co-pious white precipitates.

M. A piece of marble being boiled for some time in a few ounces of the water, the marble was found to have un-dergone no sensible loss of weight by this operation; but its surface had acquired a faint yellowish tinge.

N. A quantity of the water being evaporated to dryness, and a considerable degree of heat applied to the dry residue, a solution of this in water had the same effect of reddening litmus as before.

SECT. IV. *Inferences arising from these Effects.*

1. From experiment A, connected with experiments C, H, I, M and N, and from the circumstance of taste, and other general properties, it appeared highly probable, that the water contained sulphate of iron, and perhaps also sul-phate of alumine, without any uncombined acid*. Inferences from these.

2. From experiments C and D, it appeared evident that iron and lime were contained in the water, and that their solvent was not carbonic acid†.

3. The experiments D and E concurred to show, that the water did not contain any sensible quantity of car-bonates.

4. The experiments F and G afforded additional evi-dence of the presence of iron, and, while they showed the existence of lime in the water, seemed to indicate, that the quantity of this earth was not considerable.

* Solutions of sulphate of iron, and sulphate of alumine, though made from these salts in their crystallized state, have, like acids, the power of imparting a red colour to litmus.

† The reddish flakes mentioned in C and D, and in Sect. II, *a*, are uniformly found to be sub-sulphate of iron.

5. It

5. It appeared probable from experiment K, that the water contained a small quantity of muriatic acid.

6. The change produced in experiment B, on the infusion of Brazil-wood, appeared at first ambiguous; it could not be owing to the prevalence of an alkali or carbonated earth, since the water turned litmus red, and since the presence of carbonated earths had been disproved by other results. But having found by comparative trials, that solutions of sulphate of iron changed paper stained with infusions of Brazil-wood to a black, or at least intensely dark violet colour, and that solutions of alum turned it crimson; and observing that a mixture of these solutions produced a dark purple hue, the appearance in question was easily explained.

7. The result of experiment L indicated the presence of sulphuric acid.

8. Upon the whole, and from a review of the foregoing experiments, the substances which, at this early stage of the analysis, the water appeared most likely to contain, were *sulphate of iron*, *sulphate of alumine*, *sulphate of lime*, and a small quantity of *muriatic salts*. Some sulphate of magnesia, and some alkaline sulphates, might possibly be contained in the water, though their presence could not be satisfactorily ascertained by these preliminary experiments.

Sect. V. *Gaseous contents of the Water.*

Gaseous contents of the water.

A quantity of the water measuring ten cubic inches, being boiled briskly over mercury, the gas given out, together with the air contained in the apparatus, was received in a graduated tube; on admitting caustic alkali into the tube, one tenth of a cubic inch of gas was absorbed. It appears therefore, that one hundred cubic inches of the water contain one cubic inch of carbonic acid gas, which is equivalent to about three tenths of a cubic inch to each pint. The water was uncorked at the moment of being examined, but I had not an opportunity of ascertaining the quantity of gas.

Sect. VI. *Evaporation of the Water, and Estimation of the Quantity of solid Ingredients.*

Quantity of solid ingredi-

1. Sixteen ounces of the water by measure, being evaporated down to a soft mass over a lamp, and afterward desiccated

cated in a drying apparatus at the heat of 180°*, the solid mass weighed eighty-six grains. During the evaporation the same appearances were observed as have been already related (in Sect. III, D,) and the dry saline mass assumed a pale greenish colour. On standing in the air, it slightly deliquesced, and its colour became somewhat darker. This saline mass, though slowly evaporated, never assumed a distinct crystalline appearance. ents left by evaporation.

2. I have stated before (Sect. II, d.) that some difference prevailed in the specific gravity of the several specimens of the water which were examined. A similar want of uniformity was observed in regard to the quantity of solid ingredients, as will appear from the following statement†.

	Grains.	
The 1st specimen yielded	86·	In the pint of sixteen ounces.
2d	92·	
3d	63·6	
4th	80·4	
5th	82·8	
6th	77·2	
7th	84· ‡	
8th	78·	
	644	

These eight results therefore give 80·5 grs dried at 180°, as the average quantity of solid ingredients in each pint of the water.

* This is the heat I have usually employed for desiccation, because it is that which is obtained by the water-bath which I use, and can scarcely be raised higher by that apparatus. By a heat of 180° however, I generally mean some intermediate point between 170° and 180°, for it is impossible to regulate the temperature with perfect accuracy.

† In the first of these trials, a whole pint was evaporated; but in the subsequent ones, the quantity of water was diminished to eight, six, and sometimes only four ounces, all of which, for the sake of uniformity, I have reduced in the table to the common standard of the pint.

‡ This specimen I brought myself from the spring; the others were sent me in sealed bottles from the Isle of Wight.

SECT.

SECT. VII. *Of the different Methods of Analysis applicable to the present Inquiry.*

Different methods of analysis.

In analysing a mineral water, two modes of proceeding occur from the very first. We may either evaporate the water first, and apply our reagents to the solid residue; or operate at once upon the water itself. The former plan is in general found expedient when the quantity of the solid contents of the water is small; but when, as in the present instance, the impregnation is considerable, it may be more convenient to adopt the latter method. But at all events, as the redissolution of the solid residue, when the first mode of proceeding is resorted to, generally requires the introduction of an acid, which may modify or complicate the process, it is always desirable, that both methods should be tried in succession, in order to obtain comparative results.

We may also, if necessary, precipitate from the same portion of the water the several ingredients which it contains, by applying to it in succession their respective reagents; or, if our supply be considerable, we may use a fresh portion of it for each successive operation, a mode of proceeding which is generally preferable. No difficulty being experienced during the present inquiry in regard to the supply of water, a variety of methods was tried, with the details of which I shall not trouble the Society: but in order to convey a general idea of them, and in hopes that a summary review of this kind may afford some assistance to chemical inquirers not yet accustomed to researches of this nature, I shall briefly enumerate the different plans which presented themselves at this period of the analysis, and it will be seen afterward how these plans were gradually modified.

Methods employed.

1st method.

1st method. To precipitate in succession from a known quantity of the water, the *iron* by prussiate of potash—the *lime* by oxalate of ammonia—the *alumine* and *magnesia* by caustic potash, which, by boiling, redissolves the alumine and leaves the magnesia untouched.

2d method.

2d method. To precipitate the *iron* and *earths* by subcarbonate of ammonia. To evaporate the remaining clear solution to dryness, and apply a red heat. To redissolve this

this saline residue, and evaporate the solution slowly, in order to discover any fixed *alkaline sulphate* or *muriate* which may exist in the water. To boil in caustic potash the precipitate containing the iron and earths, in order to separate the *alumine* and *silica.* To dissolve the remaining mass (supposed to contain iron, lime, and magnesia) in nitric acid, evaporate to dryness, and apply a red heat, in order to render the peroxide of iron thus formed insoluble in acid. To add to the mass, minutely pulverized, nitric or acetic acid, as either of these acids will only dissolve the *lime* and *magnesia*, which may be separately obtained by their respective reagents. And lastly, to ascertain the quantity of *oxide of iron*, supposed to have been left untouched by the acid.

3d method. To precipitate from another portion of water the iron, lime, alumine, and silica, by a solution of neutral carbonate of ammonia, which reagent retains the magnesia in solution. To boil the precipitate in caustic potash, which takes up the *alumine* and *silica.* To redissolve in muriatic acid the residue not taken up by potash, which consists of lime and iron—separate the *iron* by pure ammonia, and the *lime* by oxalate of ammonia*. Precipitate the *magnesia*† from the clear solution by an alkaline phosphate. 3d method.

4th method. To evaporate to dryness a known quantity of the water, and to boil the residue in caustic potash, which will dissolve the *alumine* and *silica*, both of which may be precipitated again by muriate of ammonia‡. Treat the residue, insoluble in potash and supposed to contain *iron*, *lime* and *magnesia*, in the manner pointed out in the 2d method. 4th method.

* It is necessary to precipitate the iron before the lime, whenever any considerable quantity of sulphate or muriate of iron is present. For oxalate of ammonia acts upon solutions of iron, as will be fully explained under the head of sulphate of lime.

† The magnesia might be equally, and perhaps more conveniently separated, by boiling a known quantity of the solid residue in the neutral carbonate of ammonia, instead of applying this reagent to the water itself.

‡ The mode in which the silica may be separated from the alumine will be detailed in a subsequent part of this paper.

5th

5th method.

5th method. After having obtained by the preceding methods a knowledge of the proportions of iron and earthy substances, and formed an estimate of the nature and quantities of acids with which they are united, to ascertain in a direct manner the quantities of acids by their respective reagents, with a view to obtain a confirmation of the preceding results.

6th method.

6th method. To boil a known quantity of the water in succinate of ammonia, till all the *iron* and *alumine* are precipitated—edulcorate, precipitate and separate the alumine from the iron by boiling in caustic potash. From the clear concentrated fluid, to separate the *lime* by oxalate of ammonia, and the *magnesia* by pure ammonia; to evaporate the remaining clear fluid to dryness, and to apply a red heat, in order to burn or volatilize any remaining portions of the tests used in the processes above described. To redissolve the residue in order to ascertain by subsequent evaporation the presence and quantity of sulphate and muriate of soda*.

7th method.

7th method. To boil a known quantity of residue of the water in alcohol, in order to ascertain what salts it may contain, which are soluble in this menstruum.

Although I found it expedient, before advancing farther in the examination of the water, and in order to regulate my steps in the progress of the inquiry, thus to trace the various plans which seemed adapted to the purpose, yet I apprehend it would be superfluous to detail here in regular succession all the trials, which arose from these different methods. I shall therefore confine myself to such as belong more immediately to my object; and in relating them, shall consider singly, and under separate heads, the various ingredients of the water, stating, as I proceed, the proportions in which they were ultimately obtained.

SECT. VIII. *Sulphate of iron.*

Prussiate of potash does not ascertain the quantity of iron.

The presence of iron, in the state of sulphate, having been abundantly proved by the preliminary experiments, the next step was, to ascertain the proportion of this salt in a given quantity of the water. The first reagent which I tried

* This process is liable to an objection, which will be hereafter fully stated, namely, that muriate of soda is decomposed by sulphate of ammonia at a high temperature.

for

for this purpose was prussiate of potash; but after many trials (which afforded uncertain and discordant results, I convinced myself, that this test, however useful for detecting the *presence* of iron, is quite inappropriate when our object is to ascertain the *quantity* of that substance*.

Fifty grains of residue† dried at the temperature of between 170 and 180, (as described in sect. VI,) and therefore equal to ten ounces of the water, were boiled in successive solutions of the potash, so as to saturate all the acid contained in that residue, and to dissolve the alumine. The remaining solid residue, which had passed first to a dark green, and some hours afterward to a dark brown or nearly black colour, was dissolved in nitric acid, and the solution evaporated to dryness, after which a red heat was applied, in order to bring the iron to a state of peroxide, and thus render it insoluble in the same acid. The mass being now treated with nitric acid, in order to separate the lime and magnesia supposed to be mixed with the oxide of iron, and the whole being thrown into a filter, the clear solution was found still to contain a good deal of iron. This last solution was, like the former, evaporated to dryness, and to the resi-

Residuum of the water boiled with potash,

treated with nitric acid

* Prussiate of potash, as a precipitant of iron, is liable to the following objections :—

Objections to prussiate of potash.

1st. It is apt, although apparently well prepared and crystallized, to precipitate certain earthy substances, and in particular alumine; this I found distinctly to happen in two experiments, in which the mixture was heated.

2dly. If the solutions be used cold, and if the metal be not highly oxidated, some of the Prussian blue unavoidably passes through the filters; or if no filters be used, it subsides but slowly and imperfectly.

3dly. If the solutions be heated, the prussiate of potash is itself decomposed, and yields a quantity of oxide of iron, which vitiates the results.

† By the word *residue*, thus generally used, is always meant the residue of the water under examination, dried at the temperature of between 170° and 180°. And in comparing a quantity of residue with a corresponding portion of the water, the average proportion of 80·5 grs for each pint (sect. VI, 2) is always assumed as the standard of comparison.

due,

and with acetic,

due, again heated to redness, acetic*, instead of nitric acid, was this time added, and the solution filtered. The filtered fluid still contained a quantity of iron, which, however, from subsequent examination appeared very inconsiderable.

and the iron reduced.

The oxide of iron left in the filter being roasted with wax and heated to redness, in order to bring it to a uniform state of oxidation, weighed 6·8 grains†.

Residuum treated with carbonate of ammonia,

2. With a view to repeat and vary the last experiment, another portion of residue, also weighing 50 grains, was thrown into a solution of neutral carbonate of ammonia, the quantity of the latter being more than sufficient to saturate any acid present, and to dissolve the magnesia suspected to exist in that residue. A considerable effervescence took place. The mixture, after this, was gently heated and filtered. The residue left in the filter was of a pale yellowish brown colour. The clear solution deposited

the filtered matter treated with potash,

a small quantity of precipitate similar to the residue left in the filter, to which residue this precipitate was added. The contents of the filter were then treated with potash, in the manner before described (sect. VIII, 1r), in order to sepa-

* The acetic acid, as well as the nitric, is said to be incapable of dissolving any iron, which has been peroxidated by the process just described. In this instance a few particles of oxide were taken up by the acid: but it is probable, that if, instead of heating the residue to redness only a few minutes, the oxide had continued exposed to a red heat for half an hour or more, the whole of it would have become insoluble.

State of the oxide.

† It may be asked, in what state of oxidation the iron is after this operation? It has generally been supposed to be reduced to the state of protoxide in consequence of the affinity of the combustible matter for oxigen; but in an experiment, which I made some years ago to ascertain this point, (the particulars of which may be seen in my account of the Brighton chalybeate) this process appeared to bring the iron to the state of peroxide; for 100 parts of iron gave 147·6 parts of oxide, proportions which are now considered as constituting the red oxide of iron. And as a confirmation of this, I observe, that Dr. Thomson, in his valuable paper on the oxides of iron, published in the twenty-seventh volume of Nicholson's Journal, states (p. 379) that some of the red oxide being mixed with oil and heated to redness, till it became black and magnetic, no diminution of weight took place. Indeed I have always obtained by this process, not a black, but a brown oxide, which in cooling passes to a red brown colour, somewhat varying in shade, but mostly resembling powdered cinnamon, and being more or less magnetic.

rate

fate the alumine, after which the residue, now supposed to contain nothing but carbonate of lime and iron, was treated with dilute muriatic acid, which dissolved it with effervescence. From this solution the lime was precipitated by oxalate of ammonia, and the remaining liquor, now containing nothing but muriate of iron, was treated with carbonate of ammonia, so as to precipitate the whole of the iron, which, in subsiding, assumed a pale reddish colour. The clear fluid being decanted off, and the precipitate carefully washed, dried, and ultimately heated to redness with a little wax in a platina crucible, weighed 7·2 grs.

dissolved in muriatic acid, precipitated by oxalate of ammonia and the iron thrown down by carbonate of ammonia.

3. It will be observed, that between this and the former result there was a difference of 0·4 grs in the quantity of oxide of iron contained in 50 grs of residue. But when it is considered, that in the first of these analyses a small quantity of iron was positively detected in the acetic solution, which, from the best estimate I could make, would have brought the quantity of iron very near that obtained in the second process, it will readily be admitted, that the coincidence was such as to authorise me to consider the last result as sufficiently accurate*.

Difference of results.

4. If therefore we consider 7·2 grs of peroxide of iron, as the quantity of this metal contained in 50 grs of the residue, which corresponds to 11·59 grs of the oxide for 80·5 grs of residue (that is for each pint of the water according to the average before established, sect. VII, 2), we shall be able so infer the quantity of sulphate of iron contained in the water.

Proportion of oxide in sulphate of iron.

5. In order to do this, however, it was necessary to ascertain by a comparative experiment the proportion of oxide, which a known quantity of sulphate of iron yields by a process similar to that which I have just described. For

Proportion of oxide in sulphate of iron.

* In one experiment in which the iron was precipitated from a similar quantity of residue, by prussiate of potash, and the prussiate of iron roasted with wax, the quantity of oxide obtained amounted to 11 grs, from which I infer, either, that a portion of the oxide of iron, always contained in prussiate of potash, must have been precipitated with the Prussian blue, or, that the prussiate of iron was not completely decomposed in the process in question, or that some earthy substance was precipitated along with the iron.

this purpose, 50 grs of transparent crystallized green sulphate of iron were dissolved in water, and treated with carbonate of ammonia as long as any precipitate appeared. This precipitate, after being carefully separated, edulcorated, dried, and ultimately heated to redness with wax in a platina crucible, weighed exactly 14 grs. It appeared in the form of a red brown magnetic powder*.

Proportion of sulphate of iron in the water.

6. Since therefore 50 grs of crystallized green sulphate of iron gave 14 grs of this oxide, the 7·2 grs of oxide obtained from 50 grs of residue, would represent 25·7 grs of green sulphate of iron; and 11·59 grs of oxide (which is the quantity contained in an English pint of the water), would represent 41·4 grs of that salt.

(To be concluded in our next.)

X.

Experiments to ascertain the State in which Spirit exists in fermented Liquors: with a Table exhibiting the relative Proportion of pure Alcohol contained in several Kinds of Wine and some other Liquors. ***By* WILLIAM THOMAS BRANDE, *Esq. F. R. S.*†**

SECT. I. IT has been a commonly received opinion, that the alcohol obtained by the distillation of wine does not exist ready formed in the liquor: but that it is principally a product of the operation, arising out of a new arrangement of its ultimate elements.

Principal proof.

The proofs which have been brought forward in support of this theory are chiefly founded on the researches of Fabroni

* This result, which was obtained in two different trials, with the variation of only 0·1 gr. corresponds exactly with the proportions given by Mr. Kirwan, in his Treatise on Mineral Waters (table iv.), in which 28 grs are the quantity of oxide stated to exist in 100 grs of green sulphate. But, in order to establish the perfect coincidence of these results, it would be necessary to know the process which Mr. Kirwan followed. The iron in his experiment is stated to have been obtained in the state of black oxide.

† Phil. Trans. for 1811, p. 337.

Fabroni‡, who attempted to separate alcohol by saturating the wine with dry subcarbonate of potash, but did not succeed, although by the same means he could detect very minute portions of alcohol, which had been purposely added.

To obtain satisfactory results from many of the following experiments, it became necessary to employ wines to which little or no spirit had been added; for very considerable addition of brandy is made to most of the common wines, even before they are imported into this country. I therefore occasionally used Burgundy, Hermitage, Cote Roti, Champagne, Frontignac, and some other French wines; to which, when of the best quality, no spirit can be added, as even the smallest proportion impairs the delicacy of their flavour, and is consequently readily detected by those who are accustomed to taste them. For these, and for the opportunity of examining many of the scarce wines enumerated in the table annexed to this paper, I am indebted to the liberality of the Right Honourable Sir Joseph Banks.

Brandy commonly added to wines.

Good French wine would be spoiled by it.

Dr. Baillie, who took considerable interest in this investigation, was also kind enough to procure for me some port wine, sent from Portugal for the express purpose of ascertaining how long it would remain sound, without any addition whatever of spirit having been made to it.

Port procured purposely without it.

Lastly, I employed raisin wine, which had been fermented without the addition of spirit.

Raisin Wine.

At a very early period of the present inquiry, I ascertained by the following experiments, that the separation of the alcohol by means of subcarbonate of potash was interfered with, and often wholly prevented by some of the other ingredients of the wine.

Insufficiency of subcarbonate of potash to separate the alcohol.

A pint of port wine was put into a retort placed in a sand heat, and eight fluid ounces were distilled over, which, by saturation with dry subcarbonate of potash, afforded about three fluid ounces of tolerably pure spirit floating on the surface.

Port wine distilled, and the spirit separated,

I repeated this distillation precisely under the same circumstances, and mixed the distilled liquor with the residuum in the retort, conceiving, that, if the spirit were a product, I now should have no difficulty in separating it

but it could not when mixen with the residuum.

‡ Ann. de Chim. vol. XXXI, p. 303.

from the wine by the addition of subcarbonate of potash; but, although every precaution was taken, no spirit separated; a portion of the subcarbonate, in combination with some of the ingredients of the wine, formed a gelatinous compound, and thus prevented the appearance of the alcohol.

Fabroni's experiments did not succeed with the author.

It has been remarked by Fabroni, in the Memoir above quoted, that one hundredth part of alcohol purposely added to wine may be separated by subcarbonate of potash; but several repetitions of the experiment have not enabled me to verify this result: when however a considerable addition of alcohol has been made to the wine, a part of it may be again obtained by saturation with the subcarbonate. The necessary addition of spirit to port wine, for this purpose, will be seen by the following experiments.

Subcarbonate of potash added to Port,

Four ounces of dry and warm subcarbonate of potash were added to eight fluid ounces of port wine, which was previously ascertained to afford by distillization 20 per cent of alcohol (by measure) of the specific gravity of 0·825 at 60°.

and the alcohol remained mixed with extract, &c.

In twenty-four hours the mixture had separated into two distinct portions; at the bottom of the vessel was a strong solution of the subcarbonate, upon which floated a gelatinous substance, of such consistency as to prevent the escape of the liquor beneath when the vessel was inverted, and which appeared to contain the alcohol of the wine, with the principal part of the extract, tan, and colouring matter, some of the subcarbonate, and a portion of water; but as these experiments relate chiefly to the spirit contained in wine, the other ingredients were not minutely examined.

One part of alcohol added to seven of wine, none could be separated.

To seven fluid ounces of the same wine, I added one fluid ounce of alcohol (specific gravity 0·825), and the same quantity of the subcarbonate of potash as in the last experiment; but after twenty-four hours had elapsed, no distinct separation of the alcohol had taken place.

One part of alcohol to three of wine: part separated.

When two fluid ounces of alcohol were added to six fluid ounces of the wine, and the mixture allowed to remain undisturbed for the same length of time as in the former experiments, a stratum of impure alcohol, of about a quarter of an inch in thickness, separated on the surface.

Three parts of alcohol to five wine.

The addition of three fluid ounces of the alcohol to five fluid ounces of the wine, formed a mixture from which a

quantity

quantity of spirit readily separated on the surface, when the subcarbonate was added, and the gelatinous compound sunk nearly to the bottom of the vessel, there being below it a strong solution of the subcarbonate.

Madeira and Sherry.

When in these experiments Madeira and Sherry were employed instead of Port wine, the results were nearly similar.

Previous separation of the acid made no difference.

It was suggested to me by Dr. Wollaston, that, if the wine were previously deprived of its acid, the subsequent separation of the alcohol, by means of potash, might be less interfered with. I therefore added, to eight fluid ounces of port wine, a sufficient quantity of carbonate of lime to saturate the acid, and separated the insoluble compounds produced by means of a filter. The addition of potash rendered the filtered liquor turbid, some soluble salt of lime, probably the malate, having passed through the paper; but the separation of alcohol was as indistinct, as in the experiments just related.

Lime water will not separate the acids and colouring matter perfectly.

It is commonly stated, that the addition of lime water to wine not only forms insoluble compounds with the acids, but also with the colouring matter, and that these ingredients may be thus separated without heat; but on repeating these experiments, they did not succeed, nor could I devise any mode of perfectly separating the acids, and the extractive and colouring matter (excepting by distillation), which did not interfere with the alcohol.

Whether difference of temperature in distillation affects the spirit.

If the spirit afforded by the distillation of wine were a *product*, and not an *educt*, I conceived, that by performing the distillation at different temperatures, different proportions of spirit should be obtained.

The following are the experiments made to ascertain this point.

Port distilled at 200°,

Four ounces of dried muriate of lime were dissolved in eight fluid ounces of the port wine employed in the former experiments: by this addition, the boiling point of the wine, which was 190° Fahrenheit, was raised to 200°. The solution was put into a retort placed in a sand heat, and was kept boiling until four fluid ounces had passed over into the receiver, the specific gravity of which was 0·96316 at 60° Fahrenheit.* The

* It was supposed that in this experiment a small portion of muriate of

190°, The experiment was repeated with eight fluid ounces of the wine without any addition, and the same quantity was distilled over, as in the last experiment: its specific gravity at 60° Fahrenheit, was 0·96311.

in a water bath, Eight fluid ounces of the wine were distilled in a water bath; when four fluid ounces had passed over, the heat was withdrawn. The specific gravity of the liquor in the receiver was 0·96320 at 60° Fahrenheit.

and at 180°. The same quantity of the wine, as in the last experiment, was distilled at a temperature not exceeding 180° Fahrenheit. This temperature was kept up from four to five hours, for five successive days, at the end of which period, four ounces having passed into the receiver, its specific gravity at 60° was ascertained to be 0·96314.

No difference apparently in the spirit. It may be concluded, from these results, that the proportion of alcohol is not influenced by the temperature at which wine is distilled, the variation of the specific gravities in the above experiments being even less than might have been expected, when the delicacy of the operation by which they are ascertained is considered.

Attempt to separate the spirit by freezing. I have repeatedly endeavoured to separate the spirit from wine, by subjecting it to low temperatures, with a view to freeze the aqueous part; but when the temperature is sufficiently reduced, the whole of the wine forms a spongy cake of ice.

The same with a compound of alcohol. In a mixture of one fluid ounce of alcohol with three of water, I dissolved the residuary matter, afforded by evaporating four fluid ounces of Port wine, and attempted to separate the alcohol from this artificial mixture by freezing; but a spongy cake of ice was produced as in the last experiment.

But wine may be made stronger by freezing. When the temperature is more gradually reduced, and when large quantities of wine are operated upon, the separation of alcohol succeeds to a certain extent, and the portion which first freezes is principally, if not entirely water; hence in some countries this method is employed to render wine strong.

of lime might have passed over into the receiver, but the distilled liquor did not afford the slightest traces of it, to the tests of oxalate of ammonia and nitrate of silver.

SECT

SECT. II. Having ascertained, that alcohol exists in wine ready formed, and that it is not produced during distillation, I employed this process to discover the relative proportion of alcohol contained in different wines. Relative proportion of alcohol in wines.

In the following experiments, the wine was distilled in glass retorts, and the escape of any uncondensed vapour was prevented by employing sufficiently capacious receivers, well luted, and kept cold during the experiment. Mode of conducting the experiments.

By a proper management of the heat towards the end of the process, I could distil over nearly the whole of the wine without burning the residuary matter: thus, from a pint of Port wine, of Madeira, of Sherry, &c., I distilled off from fifteen fluid ounces, to fifteen fluid ounces and a half: and from the same quantity of Malaga, and other wines containing much saccharine matter, I could readily distil from fourteen to fifteen fluid ounces.

In order to ascertain the proportion of alcohol with precision, pure water was added to the distilled wine, so as nearly to make up the original measure of the wine, a very small allowance being made for the space occupied by the solid ingredients of the wine, and for the inevitable loss during the experiments: thus, five fluid drachms and a half of distilled water were added to fifteen fluid ounces and a quarter of the liquor procured by the distillation of a pint of port wine, and in other cases nearly the same proportions were observed. This mixture of the distilled wine and water was immediately transferred into a well stopped phial, and having been thoroughly agitated, was allowed to remain at rest for some hours; its specific gravity (at the temperature of 60° Fahrenheit), was then very carefully ascertained, by weighing it in a bottle holding exactly one thousand grains of distilled water at the above temperature, and the proportion of alcohol per cent, *by measure*, was estimated by a reference to Mr. Gilpin's tables*, the specific gravity of the standard alcohol being 0·82500 at 60°.

As the most convenient mode of exhibiting the results of these numerous experiments, I have thrown them into the form of a table; in the first column the wine is specified; the second contains its specific gravity after distillation, as

* Phil. Trans. 1794.

above

above described; and the third exhibits the proportion of the pure spirit, which every hundred parts of the wine contain. I have also inserted porter, ale, cider*, brandy, and some other spirituous liquors, for the convenience of comparing their strength with that of the wines.

Proportion of alcohol in various fermented liquors.

Wine.	Specific Gravity after Distillation.	Proportion of Alcohol, per Cent, by Measure.
Port	0·97616	21·40
Ditto	0·97532	22·30
Ditto	0·97430	23·39
Ditto	0·97400	23·71
Ditto	0·97346	24·29
Ditto	9·97200	25·83
Madeira	0·97810	19·34
Ditto	0·97616	21·40
Ditto	0·97380	23·93
Ditto	0·97333	24·42
Sherry	0·97913	18·25
Ditto	0·97862	18·79
Ditto	0·97765	19·81
Ditto	0·97700	19·83
Claret	0·98440	12·91
Ditto	0·98320	14·08
Ditto	0·98092	16·32
Calcavella	0·97920	18·10
Lisbon	0·97846	18·94
Malaga	0·98000	17·26
Bucellas	0·97890	18·49
Red Madeira	0·97899	18·40
Malmsey Madeira	0·98090	16·40
Marsala	0·97196	25·87
Ditto	0·98000	17·26
Red Champagne ..	0·98608	11·30
White Champagne	0·98450	12·80
Burgundy	0·98300	14·53
Ditto	0·98540	11·95

* The proportion of spirit, which may be obtained from these three liquors, is subject to considerable variation in different samples: the number given for each, in the table, is therefore the mean of several experiments, as it did not seem necessary to specify them separately.

White

Wine.	Specific Gravity after Distillation.	Proportion of Alcohol, per Cent. by Measure.
White Hermitage	0·97990	17·43
Red Hermitage	0·98495	12·32
Hock	0·98290	14·37
Ditto	0·98873	8·88
Vin de Grave	0·98450	12·80
Frontignac	0·98452	12·79
Cote Roti	0·98495	12·32
Rousillon	0·98005	17·26
Cape Madeira	0·97924	18·11
Cape Muschat	0·97913	18·25
Constantia	0·97770	19·75
Tent	0·98399	13·30
Sheraaz	0·98176	15·52
Syracuse	0·98200	15·28
Nice	0·98263	14·63
Tokay	0·98760	9·88
Raisin Wine	0·97205	25·77
Grape Wine	0·97925	18·11
Currant Wine	0·97696	20·55
Gooseberry Wine	0·98550	11·84
Elder Wine	0·98760	9·87
Cyder	0·98760	9.87
Perry	0·98760	9·87
Brown Stout	0·99116	6·80
Ale	0·98873	8·88
Brandy	0·93544	53·39
Rum	0·93494	53·68
Hollands	0·93855	51·60

Proportion of alcohol in various fermented liquors.

XI.

Meteorological Table for 1811. *In a Letter from the Right Hon.* W. J. LORD GRAY.

To WILLIAM NICHOLSON, Esq.

SIR,

THE inclosed Table is the result of last year's observations at Gordon Castle, the residence of the Duke of Gordon; which, if you choose, you may publish.

Meteorological table.

It

Improvement of the science.

It would facilitate the promotion of meteorological science very much, if registers of the state of the atmosphere were more generally kept, and the results more publicly diffused.

I remain, sir,

Your most obedient humble servant,

TWICKENHAM,
17th of April, 1812.

GRAY.

Meteorological Table, extracted from the Register kept at Gordon Castle, County of Banff, N. Britain: Latitude 57 , 38. *Above the Sea* 100 *feet.*

1811	Morning, eight o'Clock. *Mean height of*		Aftn. three o'clock. *Mean height of*	Depth of Rain.	NUMBER of DAYS.			
	Barom.	Therm.	Therm.	In. 100	Rain or snow.	Fair.	West winds.	East winds.
January.	29·79	32·64	35·06	1·34	14	17	22	8
February.	29·34	34·57	38·18	2·65	14	14	14	14
March.	29·96	40·93	48·90	0·62	6	25	24	7
April.	29·71	42·90	48·30	3·93	21	9	14	15
May.	29·80	50·90	56·68	3·64	16	15	8	23
June.	29·87	54·93	59·33	0·73	11	19	18	12
July.	29·98	57·90	61·55	2·09	17	14	22	8
August.	29·75	55·10	60·51	4·03	21	10	26	5
September.	29·97	51·50	59·23	2·53	11	19	28	2
October.	29·55	49·13	53·84	4·42	16	15	20	11
November.	29·71	42·36	44·83	2·30	20	10	27	3
December.	29·48	34·64	36·87	3·06	21	10	30	1
Average of the Year	29·74	45,62	50·27	31·34	188	177	253	109

N. B. The S. wind, and all to the W. of the meridian, are called west.

XII.

XII.

On Extract and the Saponaceous Principle; by Mr. Schrader, *of Berlin.**

After quoting the works of Rose, Hermbstaedt, Trommsdorf, Fourcroy, and Vauquelin†, all of whom have examined these two matters, the author adds: Writers on the subject.

If oxidation be the principal characteristic of extract, cinchona is the substance that should be preferred for obtaining it. Accordingly I exhausted some cinchona (*china fusca et officinalis*) by alcohol, till the menstruum was no longer coloured. The tincture obtained had no action on solution of gelatine, but it reddened litmus paper, and precipitated sulphate of iron green. Peruvian bark preferred for obtaining it. Treated with alcohol,

Having subjected the tincture to distillation, water was poured on the residuum; when a sediment formed, which was separated.

The cinchona exhausted by alcohol was treated with cold water. Litmus paper and sulphate of iron were not perceptibly affected by the impression; but it precipitated gelatine. This aqueous solution was evaporated and redissolved several times, and each time the precipitate that formed was collected. Lastly, this extract was purified from *cinchonate of lime* by alcohol, and mixed with the alcoholic tincture. cold water,

The cinchona, that had been treated successively by alcohol and cold water, was boiled in water. The brown decoction was likewise evaporated and redissolved several times, taking care to separate the flocculent matter that subsided. This sediment afforded a brown powder, having distinctly the smell of extract of cinchona; which was little soluble in boiling water, or in alcohol, but formed a and boiling water.

* Ann. de Chim. vol. LXXII, p. 290. Abridged from Gehlen's Journal by Mr. Vogel.

† See also a paper on vegetable astringents, by Dr. Bostock, Journal, vol. XXIV, p 204, 241, in which the existence of extract as a separate principle is rendered very questionable. Dr. Henry however remarks, Elements of Chemistry, vol. II, p. 181, that Dr. Bostock did not examine the extract from saffron. C.

red

red liquor with caustic lixivium. When no more sediment formed, the liquor was added to the two preceding obtained by alcohol and cold water.

The extract oxided by repeated solution and evaporation.

More than a hundred evaporations and solutions were made by the help of a water-bath. Thus the pulverulent matter was reduced to a small quantity, and the colour of the liquid became deeper. By repeated solutions almost the whole was converted into powder. Of four ounces of cinchona, that had furnished the extract, 15 grains [?] of residuum were left, on which pure alcohol would not act.

Action of extract on iron,

The infusions of cinchona then contained extract, which precipitated iron of a green colour; a property that extract loses, when it reaches the maximum of oxidation.

gelatine,

Gelatine precipitates the extract of cinchona but in part, and the supernatant liquid comports itself in the same manner as infusion of cinchona.

and tin.

Solution of tin precipitates the infusion of cinchona, but the supernatant liquid has still the same properties; for it forms a green precipitate with iron, and, when boiled some time, the extract becomes oxided, and falls down in a flocculent sediment. Tin therefore precipitates the extract only in part; and the same takes place with lime water, or with a solution of alum.

Insoluble in alcohol.

Pure alcohol does not dissolve extract; and its action is still farther diminished by the oxidation of the latter.

Distilled water.

When cinchona, or its extract, is distilled with water, the product reddens litmus, without rendering the solution of iron turbid. But if extract of cinchona be distilled, till it becomes thick, the distilled product precipitates sulphate of iron green, and comports itself like the substance of coffee.

Attempt to obtain the saponaceous principle from gentian.

To obtain the saponaceous principle, the roots of gentian and of soap-wort were treated with alcohol. The alcoholic tincture of gentian was evaporated and redissolved in water. A resinous substance was deposited. The filtered liquor reddened infusion of litmus powerfully, but did not produce a green with solution of iron.

Properties of the matter obtained.

Neither muriate of tin, lime-water, nor gelatine, rendered the liquor turbid.

The liquid extracted by weak alcohol, and afterward diluted

diluted with water, containing the saponaceous principle, was evaporated and redissolved 40 or 50 times on a water-bath. Each time a brown powder, insoluble in water or alcohol, was thrown down.

The mother water of the infusion of gentian was oxided by oxigen gas, and by oximuriatic acid. The extract from gentian therefore is less greedy of oxigen than that from cinchona, but nevertheless it becomes oxided.

Though the infusion of gentian differs from that of cinchona in not acting on tin or lime, still we cannot say, that it contains a saponaceous principle.

If extract be insoluble both in pure alcohol and in ether; and the saponaceous principle, or the substance so called, enjoy the same properties; what are the characteristics of the latter?

Root of soapwort treated in the same manner.

The root of soapwort was treated in the same manner. The infusion comported itself with gelatine and the other reagents, like that of gentian.

The same with extract.

The saponaceous principle and extract, having the same properties, it should be called therefore, agreeably to the French chemists, extract.

Principle in coffee the same.

The matter in coffee announced as a new substance by Chenevix*, and by Payssé as a new acid†, does not differ perceptibly from the extract just described.

Properties of extract.

Extract then is an immediate principle of vegetables, existing under various modifications. It combines with several metallic oxides, particularly those of tin and iron, and produces a green colour with the latter. It unites also with lime, and with alumine. It always contains nitrogen. When concentrated it exhibits a transparent mass, more or less brown, which attracts the moisture of the air. Very frequently it contains free acetic acid, muriates, and a saccharine matter.

In living vegetables it appears to be colourless; but oxigen imparts to it a black colour. This appears probable from the sap of trees, which is white when it first flows from them.

Tannin proba-

It is very probable, that tannin is a modification of extract.

* See Journal, vol. II, p. 114. † Ibid. vol. XVII, p. 126.

It

bly a modification of it.

It possesses all its properties, and in addition that of combining with glue.

General conclusions.

Thus it follows, 1, That the saponaceous principle, which has been said to have been found in several vegetables, does not exist: it is nothing but extract.

2, That extract has the property of reddening the blue colour of litmus.

3, That this substance is soluble only in water and diluted alcohol; neither pure alcohol nor ether having any action on it when well dried.

4, That when it is diluted with a great deal of water, if it be boiled in contact with air it absorbs oxigen, and falls down in a powder insoluble both in water and in alcohol.

XIII.

An Examination of the Chromate of Iron of the Uralian Mountains, in Siberia: by Mr. LAUGIER*.

Chromate of iron found in France,

IN the year 7 Mr. Pontier discovered in the department of the Var, near the mansion house of la Cassade, a mineral, which Mr. Tassaert first ascertained to be a compound of chromic acid, and oxide of iron. Mr. Vauquelin confirmed this analysis; and beside a difference in the proportions announced the presence of alumine and silex.

and in Siberia.

Mr. Meder has since found in Siberia, in the Uralian Mountains, a substance much resembling the mineral of the Var. The examination of this substance, a specimen of which was presented to the author by Mr. Steinacher, of the Society of Apothecaries at Paris, is the subject of the present paper, in which the results of the author's analysis are compared with those of Mr. Vauquelin.

Comparison of the two.

Though the Siberian mineral pretty closely resembles in appearance that from the Var, there is reason to conjecture, on examining it with attention, that the metal in it is more pure: its fracture, instead of being granular, is foliated; its metallic lustre is more vivid; and in general it is less

* Ann. de Chim. vol. LXXVIII, p. 70. Abridged from the Ann. du Muséum d'Hist. Nat. vol. VI, p. 325.

mixed

mixed with earthy matters. On some parts of its surface the specimen exhibits green spots, which may be known for oxide of crome. Its specific gravity supports the opinion of its greater purity. That of the specimen is 4·0579, while that of the mineral of the Var is only 4·0326. This difference in the gravity indicates of course a difference in the proportions of the metallic matter contained in these two varieties, and this the analysis proves.

Component parts of the Siberian mineral.

From the experiments related in the paper, which are too long to be inserted here, it follows, that the Siberian mineral contains, in 100 parts,

Oxide of chrome	53
——— iron	34
Alumine	11
Silex	1
	99.

and of the French.

These results differ a little in the proportions from the following obtained from the mineral of the Var by Mr. Vauquelin:

Chromic acid	43
Oxide of iron	34
Alumine	20
Silex	2
	99.

They are probably not chromates of iron, but compounds of the oxides of iron and of chrome.

Does chrome exist in the state of acid, or in that of oxide, in the mineral called chromate of iron? Mr. Godon de Saint-Mesmin, in a paper on the artificial combinations of chromic acid, is inclined to think, that it is in the state of oxide. Mr. Vauquelin, in his report on that paper, appears disposed to adopt the same opinion. Mr. Laugier agrees in it, and supports it by the following reflection. No direct experiment proves, that chrome is in the acid state in the native chromates of iron; and we have so much the less reason to think it, as slightly calcining the green oxide of chrome with caustic potash is sufficient, to convert it almost immediately into an acid: it is quite as probable therefore, that chrome is in the state of oxide in the minerals of the

Var

Var and of Siberia, as in that of acid; and quite as rational to consider them as compounds of the oxides of iron and of chrome, as chromates of iron.

The Siberian mineral analysed by Lowitz.

Since I have finished the examination of this mineral, adds the author, I have learned, that Mr. Lowitz has analysed the same subtance. I know not what are precisely the proportions of the principles found by him: but, to judge by the note on the subject inserted in the Journal de Physique, the results he obtained nearly agree with mine; since it says, that he found in the Siberian mineral more than half its weight of oxide of chrome, iron, alumine, and a little silex.

SCIENTIFIC NEWS.

THE Jacksonian Prize of the Royal College of Surgeons in London, for the year 1811, has been adjudged to Mr. Joseph Hodgson, of Bucklersbury, London, for his dissertation on the diseases of Arteries and Veins; comprising the pathology and treatment of Aneurism and wounded Arteries.

To Correspondents.

Mr. Lydiatt's paper is obliged to be deferred till next month, the plates for both numbers of the Journal for the present month having been some time in hand, when it was received.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1812.

ARTICLE I.

A Description of the Smicrologometer for ascertaining the Tenacity of Metals, Silk, Cotton, and Linen Threads, &c. invented by Mr. E. LYDIATT, *Professor of Mechanics, and Lecturer on Metallurgy and Manufactures, &c.*

To W. NICHOLSON, Esq.

SIR,

IN the course of much practice in the application of metals to the purposes of delicate machinery, I have frequently found it necessary to ascertain their comparative tenacity; and also of alloys, in different proportions of combination.

Tenacity of metals desirable to be known,

To do this, I have adopted the process employed by Muschenbroeck and other foreign experimenters on this subject; that is, by drawing the metals, to be subjected to experiment, into wires of a given thickness, and then suspending them vertically by one extremity, while weights were attached to the other; which weights were increased by fractional additions, till a separation of the particles took place.

and sought by the suspension of weights from wires.

The results obtained, however, by these experiments, I never found satisfactorily correct, on account of the impossibilily of increasing the weight attached by sufficiently

The results not satisfactory.

small proportions, as not in most instances to exceed what was absolutely necessary to effect the required purpose.

The spring steelyard defective.

I began therefore to consider, that the employment of a spring might be more effectual for my purpose than weights. I according tried a common spring steelyard, which, however, I found objectionable, from the difficulty of noting the breaking point precisely on the graduated slide, by reason of its being instantantaneously carried back by the recoil of the spring: an objection affecting the accuracy of the experiment as materially as that above stated against the weights.

A screw combined with the spring effectual.

After a little farther consideration on the subject, I contrived a method of combining a screw with the spring, in a way which I have found effectually to give a force capable of being approximated to the required point by the most uniform and gradual increments; constituting thereby an experimental accuracy, which I believe impossible to be obtained from weights however applied.

In this arrangement, by the rotary motion of the screw, an index is moved round a quadrantal scale, graduated with lbs. and their decimal parts; and this index remaining stationary at the breaking point, an accurate indication of the quantity of force is obtained, and can be noted at pleasure.

The principle of extensive application.

This contrivance being considered capable of an extensive and useful application, is the reason I solicit its publicity through the medium of your Scientific Journal.

The whole of the Smicrologometer is represented in the annexed drawing, which I hope will be rendered intelligible by a short explanation.

The apparatus described.

Fig 1, Plate III, is a perspective view.—AA a piece of wood of any required length and thickness, at one extremity of which is screwed the brass plate BB. On this are fixed the two standards CC; and into these is pivotted the endless screw *d*, furnished with the nut *e*, which, on turning the screw by the micrometer head *f*, moves backward and forward at pleasure.

This nut has a slider *g* attached to it under the screw, and passing through a hole in the standard C, in which it slides freely as the nut is moved.—*h*, an immovable round piece

piece of brass passing through a hole in the lower extremity of the nut, and having its ends secured in the standards C C, is intended to steady the motion. G the tube containing the spiral spring represented fig. 2. Down the middle of this spring passes the rod or slide, fig. 3. *a* is a button of brass of the same diameter as the interior of the tube G, and screwed on the end of the slide, so that, when in its proper situation, it may rest on the extremity of the spring. *b*, fig. 3, a round piece of brass, which screws in and closes the end of the tube after the spring is in; it has a perforation in the middle, through which the extremity *c* of the slide freely passes, and is connected to the slider *g* of the nut *e*, fig. 1.

The apparatus described.

By this arrangement it will be seen, that as the tube G, containing the spring, is attached to the movable nut *e*, and freely supported by the end of the brass plate B B, which is turnedup at a right angle for this purpose, it must move altogether backward or forward as the nut is moved; but if the wire *i* have one end coiled round the pin *k*, which is made to turn for this purpose in a piece of brass screwed into the end of the tube, and the other round a similar pin *l*, which is inserted in a brass slider, that moves horizontally, through the standard *o* for the purpose of adjusting its length; and, while in this position, if the screw *f* be turned so as to move the nut back, the slide will be drawn out, and the tube held in its original position by the wire *i*; which will acquire a tension equal to the resisting power of the spring, as compressed between the round piece of brass *b* and the button *a* fig. 3. If the screw continue to be turned slowly, the tension of the wire will consequently increase, till the cohesive attraction of its particles be overcome by the expansive force of the spring.

This expansive force, being the measure of tenacity in the wire, will be indicated by the index *m*, consisting of a small brass quadrant and pointer as at fig. 4, fixed on the quadrantal scale *n n n*, round which it moves as the screw turns, in consequence of teeth on the edge, which work in the threads of the screw. The index moving in this way is not influenced by the recoil of the tube, when the wire breaks, but remains at the degree it has been carried to on

the scale, till the screw is turned the contrary way to bring it back to the zero. Figs 5, 6, 7, &c. represent such parts as may not perhaps be quite intelligible in the perspective view of the instrument: namely the screw and nut, standards, &c., all of which, I hope, will be understood, without a more diffuse description.

Applicable to threads or cords of any kind:

The application of the smicrologometer to ascertain the tenacity of metals being understood, it will be easy to conceive, that it may be readily employed in the same way, to determine the strength of silk, cotton, or linen threads, &c., affording thereby means of calculating with facility the force any combination of them will sustain in cordage, cloth, &c.

and to stringed instruments of music.

It will likewise be found a desideratum to those manufacturers of stringed musical instruments, who wish to approximate to perfection on scientific principles.

It being now determined by experiment, that the transmission of clear and continuous sounds from piano fortes, harps, &c., depends very much on the due proportion of their component parts, and more particularly those to which the wires or strings are immediately attached; it becomes in consequence necessary, that the exact tension, of strings producing different sounds should be known as correctly as their lengths, in order, that such proportions may be given as will exactly support the aggregate tension without impeding the vibrations by unnecessary quantities, of metal or wood.

Mode of application to these.

To adapt the smicrologometer to this purpose, nothing is necessary, but to affix a monochord scale with a movable bridge on the top of the piece of wood A A: when you have an instrument that will determine at once the length and tension of any string, or wire, to the highest degree of accuracy, that is capable of practical application.

Experiments with the instrument promised.

Satisfied that every invention and discovery, having the prospect of opening a shorter and less intricate avenue to truth, comes with a fair claim to approbation from all who are interested in the advancement of science; I shall not hesitate to lay before the public, in some future paper, the results of a number of experiments made to ascertain, more correctly than has hitherto been done, the relative tenacity of the different metals, and their alloys: accompanied with

such

such practical observations on their general properties, as may serve to show the importance of a more particular attention to this subject, than has hitherto been thought necessary in practical mechanics.

E. LYDIATT.

London, April the 15th, 1812.

II.

A Chemical Account of an Aluminous Chalybeate Spring in the Isle of Wight. By ALEXANDER MARCET, *M. D. F. R. S. one of the Physicians to Guy's Hospital, and Member of the Geological Society.*

(Concluded from p. 66.)

SECT. IX. *Sulphate of Alumine.*

1. FIFTY grains of residue † were boiled in two successive lixivia of caustic potash (as in sect VIII, 1), so as to take up all the alumine present; the residue was separated and well washed, and the washings were added to the alkaline solution. The clear liquor had a brownish colour, and on being tried with muriatic acid and prussiate of potash, a blue tinge was produced, which appeared to have arisen from a few particles of oxide of iron, which were suspended in the lixivium rather than actually dissolved; for the solution being left at rest for some time, these particles subsided. Residue treated with caustic potash,

2. To the clear alkaline solution muriate of ammonia was added, till no farther precipitate took place; the precipitate was edulcorated and collected in a filter. It was and precipitated by muriate of ammonia.

† These fifty grains had been previously boiled in neutral carbonate of ammonia, in order to separate the magnesia, as will be detailed hereafter. The previous intervention of a carbonated alkali renders the subsequent application of caustic potash for the separation of the alumine more unexceptionable, as a solution of caustic potash might redissolve a small portion of the lime, if it were not previously carbonated.

white

white and gelatinous. Caustic potash being added to the clear fluid, ammonia was disengaged, showing that it contained an excess of muriate of ammonia; and acetic acid being added to another portion of the same liquor, no turbidness appeared, both circumstances showing, that all the alumine was precipitated. This precipitate being dissolved in muriatic acid, in order to separate a minute portion of silica, which it contained*, and being again precipitated by succinate of ammonia with excess of ammonia, formed a gelatinous mass, which being edulcorated, dried, and ultimately heated to redness, weighed 2·4 grains.

Another portion.

3. Another portion of residue, weighing thirty grains, being treated in a manner exactly similar to that just described; with this exception, that the redissolution of the alumine in muriatic acid and its subsequent precipitation by succinate of ammonia, were omitted; the gelatinous precipitate, heated to redness, weighed 1·4 grain †, which afforded as close a coincidence with the former result as may be well expected in processes of this kind.

Crystals of alum obtained by adding potash.

4. Having never been able to obtain, by the mere evaporation of the water, any appearance of crystals resembling alum, I was desirous for the sake of obtaining farther evidence on the subject, to bring the sulphate of alumine to a crystallized state, by artificially supplying what I conceived to be wanting for the completion of that process. For this purpose, having dissolved about thirty grains of residue in distilled water, I added to the filtered solution two or three drops of a solution of carbonate of potash, and evaporated it very slowly; crystals were thus obtained, dispersed in the saline mass, which, though of a size scarcely exceeding that of a pin's head, had a distinct octohedral

* The particulars of the manner in which the silica is separated, by the intervention of muriatic acid, will be detailed under the head Silica, in another part of this paper.

† The real weight was 1·6 grain, but 0·2 of a grain were deducted, on account of the quantity of silica known, by other experiments, to have been present, as will be seen under the head Silica. It may be proper to mention, that the gelatinous precipitate, during its gradual desiccation, shrunk into small fragments resembling coarsely pulverized glue, an appearance which is well known to characterize alumine.

form

form, and, when separated and chemically examined, had all the properties of alum.

5. With regard to the proportion of sulphate of alumine, contained in the water, it will be seen, that by connecting together the results of the experiments just related (1, 2, 3), eighty grains of residue, or a pint of the water, yield 3·8 grains of alumine heated to redness, which, according to the proportion of twelve parts of iginted alumine in one hundred parts of crystallized alum*, would be equivalent to 31.6 grains of the alum in each pint of the water‡.

Proportion of sulphate of alumine.

Sect. X. *Sulphate of Lime.*

1. Some of the former experiments (sect. III, d and g) had shown, beyond all doubt, the presence of selenite; and indeed, from the general composition of the water, lime could scarcely be supposed to exist in it in any other form of combination.

Examination for sulphate of lime.

To ascertain the quantity of this substance, a variety of methods was used, the principal results of which I shall cursorily relate.

2. It would have been in vain, in this instance, to have applied, without any previous step, oxalate of ammonia, the usual test of lime, in order to obtain an accurate estimate of the quantity of lime present in the water; for as oxalic acid also acts upon iron, some ambiguity would necessarily have occurred. Indeed that oxalate of ammonia did not, in this case, react upon the lime in the manner that it usually does, had been noticed, (sect. III, f, g) in some of the preliminary experiments†.

3, It

* These are the proportions stated by Mr. Kirwan, and which I obtained myself on a former occasion (See the Analysis of the Brighton Chalybeate)

‡ It is scarcely necessary again to observe, that the sulphate of alumine contained in the water does not appear to exist there in the state of alum; but it is perhaps better to express the quantity of alumine by the quantity of alum which it would form, as the crystallized state of the salt affords a much more precise standard of comparison.

† By adding a considerable quantity of oxalate of ammonia, and concentrating the solutions by heat, the whole of the lime appeared to be precipitated, together with a portion of iron; but in order to obtain

Iron precipitated with lime by oxalate of ammonia.

Proportion of sulphate of lime.

3. It was therefore necessary to separate the iron previous to the precipitation of the lime. This was done in one instance by prussiate of potash, and in another by succinate of ammonia. I shall not trouble the society with a detail of these operations. It will be sufficient to state, that the two most unexceptionable experiments indicated the one 8 grains, and the other 8·3 grains of oxalate of lime, dried at 160°, for each pint of the water, making an average of 8·15 grains of oxalate of lime, or 10·17 grains of sulphate of lime dried at 160°; or 7·94 grains of the same salt dried at a red heat*.

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obtain the oxalate of lime pure, it was necessary to calcine the precipitate so as to drive off the oxalic acid, to redissolve the residue in muriatic acid, and to precipitate the lime again by oxalate of ammonia. The small quantity of iron present did not *then* interfere, and this process, however circuitous, proved tolerably accurate.

I was drawn by this part of the subject into an experimental inquiry respecting the action of oxalate of ammonia on solutions of iron, and the unfitness of this test for the precipitation of lime when iron is present, the principal results of which I shall state summarily.

1. If to a strong solution of sulphate of iron a small quantity of sulphate of lime be added, and then a little oxalate of ammonia, no precipitate or cloudiness appears; while the same quantities of sulphate of lime and oxalate of ammonia, added to a bulk of water equal to that of the solution of iron, instantly form a precipitate.

2. If oxalate of ammonia be added to a solution of sulphate of iron, a bright yellow colour is produced; and presently after this a copious white precipitate appears, which, in subsiding, assumes a pale lemon colour. If, at the moment the cloud is forming, the vessel be scratched with any pointed instrument, white lines appear, as in the precipitation of magnesia from carbonic acid by phosphoric acid.

3. This precipitate being washed, and gently heated over a lamp, assumes a bright cinnamon colour, and becomes magnetic, in consequence, no doubt, of the carbonization of the oxalic acid; and these changes take place at a heat much inferior to ignition.

4. If a solution of potash be added to the washed precipitate, previous to the application of heat, a strong smell of ammonia arises, and the oxide passes to a dark grayish colour, showing that the precipitate is a triple salt of oxalic acid, iron, and ammonia.

* I avail myself, in forming these various estimates, of the proportions given by Dr. Henry, in his valuable 'Analysis of several varieties of Sea Salt' (published in the Philosophical Transactions for 1810, page 114), where he states that 100 grains of ignited sulphate of lime (which

SECT. XI. *Inferences obtained from the application of Alcohol.*

1. Having ascertained (sect. III, k), that a small quantity of muriatic acid was present in the water, it became desirable, before proceeding any farther, to discover, by the agency of alcohol, which has the well known property of dissolving the earthy muriates, with what bases this acid was combined. With this view, 20 grains of residue were digested in successive quantities of alcohol of great purity, and the solution filtered. The residue, by this operation, acquired a lighter colour and a more pulverulent appearance. Part of this residue being treated with muriatic acid and oxalate of ammonia, oxalate of lime was precipitated; and another portion being treated with neutral carbonate of ammonia and phosphate of soda, some magnesia was precipitated in the form of triple phosphate, circumstances which confirmed the presence of lime in the form of selenite, and that of magnesia, in the form of sulphate or Epsom salt. Examination for muriates by alcohol.

2. The alcoholic solution being evaporated to dryness, a yellowish deliquescent residue was obtained, which, being dried at 160°, weighed 0·9 of a grain. Water being added to this residue, a small portion of it remained undissolved. The filtered watery solution was yellowish, though perfectly transparent, and, being examined by the usual reagents, appeared to contain iron, sulphuric acid, and muriatic acid, with imponderable vestiges of lime and magnesia, without any trace of alumine.

3. From these circumstances it was inferred, that the only deliquescent salts yielded by the residue, in ascertainable quantities, were sulphate of iron, and muriate of iron, both of which had probably been formed in consequence of some new orders of attraction taking place during the process of evaporation to which the water had been subjected*. Results.

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(which he finds to be equal to 128 grs. dried at 160°), give 102·5 grs. of oxalate of lime dried at 160°, corresponded to 124 grs. of sulphate of lime dried at the same temperature. [See Journ. vol. XXVI. p. 278.]

* Namely, the red sulphate from the hyperoxigenation of the iron, and the muriate from the decomposition of muriate of soda, as will be explained hereafter.

SECT. XII. *Sulphate of Magnesia.*

Examination for magnesia.

1. The presence of magnesia * was acertained beyond all doubt, in the following manner:

50 grains of residue minutely pulverized were boiled in a solution of neutral carbonate of ammonia, so as to decompose all the sulphate of iron and earthy salts, and dissolve all the magnesia which might be present†. This process was, of course, attended with considerable effervescence, and when this had subsided, the liquor was filtered. The clear solution deposited on standing a brownish sediment, which was separated, and proved to be oxide of iron. The residue left in the filter had passed from a greenish-yellow to a pale brown colour.

Indication of ammoniaco-magnesian phosphate.

2. Phosphate of ammonia being added to the clear solution, a precipitate appeared, having all the characters of the ammoniaco-magnesian phosphate; and in particular, that of forming white stripes on the inside of the vessel when scratched with a pointed instrument. This precipitate, dried at a temperature of about 120°, weighed 1·9 grain‖, and being made red hot in a platina crucible, was reduced to exactly 1 gr. = 0·385 of a grain of pure magnesia = 2·26 grains of crystallized sulphate of magnesia in 50 grains of residue, or 3·63 grains in a pint of water‡. The magnesian

Proportion of sulphate of magnesia,

* The presence of this earth in the form of sulphate had already been proved by the application of alcohol, (sect. XI, 1).

† It is scarcely necessary again to state here the well known fact, that carbonate of ammonia, when fully saturated with carbonic acid, has the power of dissolving magnesia.

‖ In a subsequent experiment, in which the water itself, instead of the residue, was treated in the same manner with neutral carbonate of ammonia, the quantity of magnesia appeared somewhat greater; but the difference did not amount to more than one tenth of a grain.

Proportion in which magnesia and phosphoric acid combine.

‡ It will be necessary here to state the grounds of this computation, which will afford me an opportunity of relating some general results concerning the proportions in which magnesia and phosphoric acid combine.

By dissolving 11·82 grains of the purest magnesia (perfectly free from carbonic acid and water) in muriatic acid, and precipitating it by a mixture of phosphate of ammonia, and neutral carbonate of ammonia, I obtained 65 8 grains of the triple phosphate dried by exposure for near forty-eight hours to a temperature which never exceeded

magnesian phosphate became slightly brownish during the calcination, owing to the presence of a few particles of iron, the quantity of which was too minute to be ascertained.

SECT. XIII. *Precipitation of the sulphuric and muriatic Acids, with a view to ascertain their quantity.*

Before drawing any ultimate conclusion respecting the contents of the water and the proportions of its ingredients, I found it necessary to ascertain the quantities of sulphuric and muriatic acids which it contained, in order to enable me to try how far these quantities might coincide with the conclusions obtained by the separation of the basis, and also to assist me, as will be seen hereafter, in forming certain inferences with regard to the alkaline salts. For this purpose I made the following experiments. Examination of the quantity of the acids.

1. To

ceeded 120°, a degree of heat under which this salt appears to retain the whole of its ammonia. These 65·8 grains of triple salt, being exposed for half an hour to a strong red heat in a platina crucible, were reduced to 30·8 grains. The salt appeared then in the form of a friable cake or loose aggregate, a fragment of which, on being urged by the blowpipe, ran into a white opaque vitreous globule, without any farther diminution of weight. In its friable state it was readily dissolved by muriatic acid; in its vitrified form it required heat and trituration. This salt was perfectly tasteless, and showed no attraction for water. With regard to the proportions of acid and base to be inferred from this experiment, it is obvious, that, if 30·8 grains of phosphate of magnesia contain 11·82 grains of earth, the remainder, viz. 18·98 grains, represents the proportion of phosphoric acid; which is equivalent to 38·37 grains of magnesia in 100 of phosphate. In another experiment conducted in a similar manner, the magnesia amounted to 38 7 grains, so that, by taking the mean between these two very nearly similar results, we have the following proportions, viz.

Magnesia . 38·5 }
Phosphoric acid 61.5 } in 100 grains of ignited phosphate of magnesia.

We may infer therefore, that one grain of phosphate of magnesia, the quantity yielded by twenty grains of residue, indicated 0·385 of pure magnesia; and if, according to the statements of Kirwan and Wenzel (which very nearly agree) one hundred grains of crystallized sulphate of magnesia contain seventeen grains of magnesia, 2·26 grains of that salt will be the quantity corresponding to 0·385 of a grain of magnesia. And I have the satisfaction of observing, that the proportions

Sulphuric acid. 1. To four ounces of the water was added nitrate of barytes till the whole of the sulphuric acid was precipitated; the sulphate of barytes thus obtained being carefully edulcorated, and heated to redness in a platina crucible, weighed 18·5 grains, which correspond to 74 grains of sulphate of barytes from a pint of the water.

Muriatic acid. 2. Four ounces of the water were treated with nitrate of silver as long as any precipitate appeared, and the muriate of silver thus obtained, being well edulcorated, and afterwards brought to a state of incipient fusion by the heat of an Argand lamp, weighed 2·05, which is equivalent to 8·2 grains of luna cornea, or four grains of muriate of soda*, in each pint of the water†.

Sect. XIV. *Sulphate and Muriate of Soda.*

Examination for alkaline salts. 1. The mode in which I first attempted to ascertain the presence of alkaline salts in the water was that alluded to in a former part of this paper, which consisted in precipitating the iron and the earths by subcarbonate of ammonia, evaporating the clear solution to dryness, heating the dry mass to redness, with a view to drive off the sulphates and muriates of ammonia, redissolving the residue in water, and evaporating again very slowly in order to obtain crystals. But the saline mass yielded by this process did not crystallize regularly, and, on being examined by reagents, was found to contain only sulphate of soda, with minute quantities of sulphates of alumine and magnesia, which had escaped the action of the carbonate of ammonia.

portion, obtained by Dr. Henry, of one hundred grains of ammoniaco-magnesian phosphate dried at 90°, for one hundred and eleven grains of crystallized sulphate of magnesia, would have led to a very similar result. (See Dr. Henry's 'Analysis of several varieties of Salt,' in Philos Trans. for 1810, page 113.) [Journ. vol. xxvi, p. 277.]

* I have found by direct experiments, that one hundred grains of pure muriate of soda heated to redness, and decomposed by nitrate of silver, yield 241·6 grains of luna cornea heated to fusion.

† The same experiment was tried three times upon different specimens of the water, and I here give the average. The smallest quantity of luna cornea obtained was two grains, and the largest 2·5 grains, a difference too great to arise from mere inaccuracy. From this and several other circumstances I have reason to suspect, that the water is subject to occasional variations in the proportions, as well as in the aggregate quantity of its solid contents.

2. In hopes of obtaining more satisfactory results, I had recourse to the following process: five ounces of the water were boiled with a solution of succinate of ammonia till the whole of the iron and alumine were precipitated*. The lime was precipitated by oxalate of ammonia, and the magnesia by ammonia. The solution was then concentrated over a lamp, and gradually evaporated to dryness in a platina crucible. A white pungent smell arose, and on raising the heat to redness, these fumes took fire and burnt with a blue flame, till the whole was fused and reduced to a fixed saline mass mixed with a black coaly matter. Distilled water was poured upon this mass, and the solution filtered. This clear solution being now evaporated and dried at a gentle heat, so as to obtain the salts in a crystallized state, the mass weighed 6·3 grains†, which would give 20 grains of alkaline salts in a pint of the water. The centre of this mass exhibited no distinct crystallization, though from its appearance and disposition to effloresce, it evidently contained sulphate of soda; but the circumference was strewed with numerous and perfectly regular crystals of muriate of soda‡.

Treatment with succinate of ammonia.

3. This

* This is a long operation, because the iron does not combine with the succinic acid at a low degree of oxigenation, so that the mixture must be long digested with access of air, or repeatedly boiled and allowed to stand in the air for some hours during the intervals, before the process can be completely effected. This operation necessarily requires one or two days, but is remarkably accurate as to the precipitation of both the iron and alumine.

† This was the combined result of two separate experiments tried on three and two ounces of the water, the first of which yielded 3·5 grains, and the other 2·8 grains of alkaline salts.

‡ This result shows the compatibility of muriate of soda with sulphate of iron, the latter being in excess, which has been questioned by some chemists. Being desirous of obtaining a confirmation of this by a direct experiment, I mixed together solutions of two parts of sulphate of iron and one part of muriate of soda. The mixture became yellowish, and on applying heat reddish flakes subsided. On separating these by filtration, and repeating this process two or three times, I nevertheless obtained by evaporation distinct crystals of muriate of soda, partly cubic, partly octohedral, deposited in the centre of a saline yellowish mass, without any appearance of efflorescence or of any thing resembling sulphate of soda. Therefore muriate of soda is compatible with sulphate of iron, although these two salts

Muriate of soda compatible with sulphate of iron.

Properties of the saline mass.

3. This saline mass being dissolved in water, the solution had the following properties:

a. It was neither acid nor alkaline.

b. Its most obvious taste was that of muriate of soda.

c. It formed copious precipitates with nitrate of barytes, nitrate of silver, and nitrate of lime.

d. Oximuriate of platina, oxalate of ammonia, and prussiate of potash, produced no precipitate whatever.

Therefore the only salts contained in this solution were sulphate of soda, and muriate of soda.

Perhaps muriate of soda only present.

4. As to the proportions of these two salts, it would have been easy to ascertain them by precipitating their acids. But it occurred to me, that the sulphate of ammonia formed in the solution by the ammoniacal salts, which had been introduced for the precipitation of the earths, had probably reacted upon the muriate of soda when urged by heat, so as to decompose it partially, and form the sulphate of soda obtained by the process just described; so that muriate of soda might perhaps in fact be the only alkaline salt contained in the water.

Proof of the action of sulphate of ammonia on muriate of soda.

5. In order to ascertain this, another portion of the chalybeate having been treated in the way just described with succinate of ammonia, the residue was gradually desiccated, and then heated to redness in a platina crucible, which was at first kept closed, in order to retard the escape of the sulphate of ammonia, and thus promote its action on the muriate of soda. The remaining mass, being dissolved and very slowly crystallized, assumed the form of clusters of regular prismatic efflorescent crystals of sulphate of soda, among which scarcely any vestige of muriate of soda could be discovered.

Proportions of the sulphate and muriate determined indirectly.

6. The decomposition of muriate of soda by the above process being thus well established, it became necessary to determine the proportions of sulphate and muriate of soda by some

salts evidently exert some degree of action on each other, as appeared from the change of colour and the formation of reddish flakes, which I suppose to be subsulphate of iron. I may take this opportunity of mentioning, that by an analogous experiment on sulphate of iron and muriate of alumine, and by the assistance of alcohol, I satisfied myself that these two salts *could not* exist together.

some less direct method; and the expedient which appeared the most appropriate was that of inferring the point in question from a reference to the quantities of acids as estimated in the preceding section. Thus as it was obvious that, whatever the case might be with regard to sulphate of soda, the presence of muriate of soda in the water was unquestionable; and as the whole quantity of muriatic acid discovered in the water (§XIII, 2), corresponded to a quantity of muriate of soda which fell far short of the sum total of alkaline salts, I naturally inferred, that the whole of the muriatic acid was united with soda, and that the water must also contain a quantity of sulphate of soda sufficient to complete the 20 grains of alkaline salts, which the experiments just related had shown to exist in each pint of the water.

7. Since therefore the whole of the muriate of soda, as was before computed (§XIII, 2), amounted only to 4 grains in a pint, the quantity of crystallized sulphate of soda contained in each pint of the water will be 16 grains.

SECT. XV. *Comparison of the quantities of Acid actually obtained from the water by precipitation, with the quantities inferred from the precipitation of the basis.*

Quantities of acid obtained by precipitation compared with that inferred from the bases.

1. It appears evident, from all that precedes, that the only acids contained in the water are the sulphuric and muriatic. The whole of the muriatic acid having been shown to exist in the form of muriate of soda, nothing farther remains to be said on this head. But it will be curious to examine how far the total amount of sulphuric acid, obtained from a portion of the water, would coincide with that which might be inferred from the quantities of bases, with which it was combined. This inquiry will give rise to the statement of certain results respecting the proportions of acid and base in some of the salts concerned, and the precipitates obtained from their decomposition, which, from their general import in chemical analysis, appear to deserve some attention.

2. It was ascertained by a direct experiment (§ XIII, 1) that the whole of the sulphuric acid, contained in a pint of the water, formed, when precipitated by a barytic salt, a quantity of sulphate of barytes, which, after being ignited, weighed 74 grains.

I shall

I shall now recapitulate the several sulphates discovered in the water, and from the quantities of each compute the quantities of barytic sulphate, which would result from their decomposition.

Sulphates in the water.

Sulphates contained in a pint of the Water.

	Sulph. of baryt. ignited.
Sulphate of iron (§VIII, 6) 41·4 grs. crystallized =	31·8 grs.*
Sulph. of alumine (§ IX, 5,) 3·8 grs. ign. alumine =	17·7 do. †
Sulph. of lime (§ X, 3) 10·17 grs. dried at 160° =	13·9 do. ‡
Sulph. of magnesia (§ XII, 2) 3·63 grs. crystal. =	4·0 do. ‖
Sulph. of soda (§ XIV, 7) 16·0 grs. crystallized =	11·6 do. §
Total amount of the sulphate of barytes	79·0 grs.

* These proportions were deduced from the following experiment; 50 grains of crystallized green sulphate of iron were dissolved in water, and nitrate of barytes was added as long as any precipitate took place. The sulphate of barytes, after being carefully edulcorated and heated to redness in a platina crucible, weighed 38.5 grs. Therefore 50 : 38·5 : : 41·4 : 31·8.

Proportions of acid and base in pure sulphate of alumine.

† It may be recollected that 3·8 grs. of ignited alumine would, according to the proportion before stated (Sect IX, 5,) correspond to 31·6 of crystallized alum. I found by a direct experiment, that 100 grs. of regular octohedral crystals of alum formed by gradual deposition from a staturated solution of common alum, being dissolved in water and precipitated by muriate of barytes, produced 88·2 grs. of ignited sulphate of barytes; so that the 31·6 grs. of alum would correspond to 27·8 grs. of the barytic sulphate. This, however, could not be an accurate estimate of the real quantity of sulphuric acid, since the sulphate of alumine does not exist in the water in the state of alum.

With a view to learn the proportions of acid and base in *pure* sulphate of alumine, I made the following attempt. A quantity of alumine (which had been prepared by precipitation from alum, redissolution in muriatic acid, and second precipitation by carbonate of ammonia, and appeared to contain no impurity except a vestige of muriatic acid), was dissolved in sulphuric acid, and the solution evaporated to siccity. When reduced to the consistence of a thick sirup, and allowed to cool, the saline mass congealed into a hard whitish deliquescent cake, capable of being pulverized. This was redissolved and reevaporated four successive times, and the last time was made redhot, in order to expel the excess of sulphuric acid, which always appeared to prevail. By this last operation a portion of the salt was decomposed and rendered insoluble in water, in spite of which the remainder still exhibited signs of acidity. The clear solution of this mass being divided into

two

3. It appears therefore, that the aggregate of the analytical results would indicate 79 grs. of ignited sulphate of barytes, instead of the 74 grs. obtained by a single direct operation. This difference I apprehend to be in a great degree owing to my estimate of the proportion of acid in sulphate of alumine being overrated, from the circumstance of not having been able to obtain a neutral sulphate of alumine in the experiment just related from which that estimate was deduced.

Difference between the sulphate of barytes calculated and obtained.

Sect. XVI. *Silica.*

Examination for silex.

1. During the various solutions of the residue in acid, I had repeatedly observed, that, beside the selenite, (the solution of which was attended with some difficulty, and re-

two equal portions, one of which was precipitated by succinate of ammonia, and the other by nitrate of barytes, yielded 4·5 grs. of ignited alumine, for 21 grs. of ignited sulphate of barytes. From which it may be inferred, that the 3·8 grs. of ignited alumine, found in a pint of the water, were combined with a quantity of acid equal to 17·7 grs. of ignited sulphate of barytes. But it is assumed in this computation, that the artificial sulphate of alumine subjected to analysis, was in the same state of combination as that which exists in the water, a supposition which may not be strictly accurate.

‡ The quantity of sulphate of barytes, produced by the precipitation of a given quantity of sulphate of lime, was ascertained in the following manner; some pulverized crystals of native selenite, apparently perfectly pure, were dissolved in water and afterwards slowly precipitated by evaporation. The object of this previous operation was to obtain the sulphate of lime in a state more fit for subsequent redissolution. Fifteen grains of this selenitic residue, dried at a red heat, were dissolved in water, slightly acidulated by muriatic acid, in order to supersede the necessity of using a large quantity of water; and the solution, after being neutralized by pure ammonia, was precipitated by muriate of barytes. The sulphate of barytes, thus obtained, weighed, after careful edulcoration and ignition in a platina crucible, 26·75 grs. which are equivalent to 175·6 grs. of sulphate of barytes for 100 grs. of ignited sulphate of lime.

Proportion of sulphate of barytes to sulphate of lime.

|| According to Dr. Henry 100 grs. of crystallized sulphate of magnesia give 111 grs. of ignited sulphate of barytes. See Philos. Trans. 1810, p. 114. [Journ. vol. xxvi, p. 278.]

§ These proportions were deduced from the following experiment: 40 grs. of crystallized sulphate of soda, being dissolved in water and precipitated by nitrate of barytes, the sulphate of barytes, well edulcorated and ignited, weighed 29·1 grs.

quired a considerable quantity of water) there always remained a small proportion of earthy matter, which resisted all solvents, caustic potash excepted. This insoluble matter, I had thought from some of the first trials, amounted to about 1 gr. in 100 of the residue; but from some subsequent experiments in which the silica was separated by caustic potash, there appeared to be reason to suppose, that this estimate was rather overrated. I shall relate the process, to which, after various trials, I gave the preference.

Silex dissolved by caustic potash, and precipitated by muriate of ammonia.

2. 50 grains of residue being boiled with very dilute muriatic acid, a white flocculent substance remained undissolved, upon which neither acid nor water could make any impression. This substance, being separated and boiled in a solution of caustic potash, readily redissolved with the exception of a few particles of highly oxidated iron, which subsided. Muriate of ammonia * being added to the clear alkaline solution in sufficient quantity to saturate the whole of the potash with muriatic acid, the white flocculent substance reappeared, which after being well washed, and heated to redness, weighed between 0·3 and 0·4 of a gr. This substance when heated with alkali ran into a vitreous globule, and muriatic acid being poured upon this, the alkali was dissolved, and the earthy matter remained untouched. It was therefore silica, the quantity of which may be estimated at 0·7 of a gr. in a pint of water†.

On

* This precipitant, which was, I believe, first proposed by Mr. Chenevix, is much more appropriate than acids, because if an excess of acid be incautiously added, the precipitate is redissolved: while with muriate of ammonia an excess of the test is attended with no inconvenience.

Another examination for silex.

† The presence of silica was also shown, and its quantity attempted to be ascertained by the following process. A portion of residue was boiled in caustic potash: this dissolved not only the silica, but also the alumine; both these earths were precipitated from the alkaline solutions by muriate of ammonia, and separated; muriatic acid being now added, both the silica and alumine were redissolved (for silica, just precipitated from its solution, and not desiccated, is soluble in acid); and this solution being evaporated to dryness on a water-bath, by which means the silica parts with its acid and becomes insoluble, the muriate of alumine was washed off by distilled water, and the silica remained undissolved. This method, though affording a very useful

SECT. XVII. *Conclusion.*

On reviewing and connecting together all the foregoing results, it appears that each pint, or sixteen-ounce measure of the aluminous chalybeate, contains the following ingredients: Ingredients in a pint of the water.

	GRAINS
Of carbonic acid gas three tenths of a cubic inch.	
Sulphate of iron, in the state of crystallized green sulphate	41·4
Sulphate of alumine, a quantity which, if brought to the state of crystallized alum, would amount to	31·6
Sulphate of lime, dried at 160°	10·1
Sulphate of magnesia, or Epsom salt, crystallized	3·6
Sulphate of soda, or Glauber's salt, crystallized	16·0
Muriate of soda, or common salt, crystallized	4·0
Silica	0·7
	107·4

I am not acquainted with any chalybeate or aluminous spring, in the chemical history of mineral waters, which can be compared, in regard to strength, with that just described. The Hartfell water, and that of the Horley-green spaw near Halifax, both of which appear to be analogous to this in their chemical composition, and were considered as the strongest impregnations of the kind, are stated by Dr. Garnett to contain, the one only about 14 grs. and the other 40 grs. of saline matter in each pint. It is analogous to the Hartfell and Horley green, but the strongest of its kind known.

No doubt therefore can be entertained, that the water, which is the subject of this essay, will be found to possess in a very eminent degree the medical properties, which are known to belong to the saline substances it contains. Indeed there appears to be in this spring rather a redundance than a deficiency of power; and it is probable, that in many instances it will be found expedient to drink the water in a diluted

useful means of discrimination, must obviously be liable to inaccuracy as to proportions, when very minute portions of silica are to be separated from considerable quantities of almine. This however was the process to which I trusted on a previous occasion (IX, 2.) to free the alumine from the silica which was mixed with it.

diluted state; while in others, when it may be desirable to take in a small compass large doses of these saline substances, it will be preferred in its native undiminished strength.

III.

Account of some new Eperiments on Wood and Charcoal: by BENJ: COUNT OF RUMFORD, *F. R. SS. L. and E. M. R. I. A. &c.**

Various kinds of wood procured for drying.

HAVING had occasion to dry several kinds of wood, to ascertain how much water was contained in them, I procured a piece of each kind six inches long and half an inch thick, and planed off some pretty thin shavings, which I kept to dry for eight days in a room, the temperature of which was constantly about 60° F. The wood had been previously drying two or three years in a joiner's workshop.

Exposed to a moderate heat in a stove.

Of each kind of shavings I took 10 gr. [154·5 grs.] which I placed on a china plate in a kind of stove made of sheet iron; and heated them moderately by a small fire under the stove for twelve hours, after which they were suffered to cool gradually during twelve hours more. The stove, being surrounded with brick-work, was still hot twelve hours after the fire had been extinguished.

Loss of weight.

On taking out the china plates in succession, and weighing the shavings anew, their weight was found to be diminished about one tenth, some a little more, others a little less. When the shavings were put into the stove, their weight was 10 gr., when taken out it was about 9. Their colour was not perceptibly altered, and they had no appearance of having been exposed to a strong heat.

Heated again.

Desirous of knowing how far the drying of wood might be carried, I replaced them all in the stove, which I heated as before, neither more nor less, for twelve hours, and afterward left to cool slowly for twelve hours.

On

* Read at the meeting of the first class of the French Institute, Dec. the 30th, 1811. This, as well as the following, is translated from the original, transmitted by the Count, and not yet published in France.

On taking out the shavings the next day, they had all changed colour more or less: from a yellowish white they had become light brown, dark brown, more or less yellow, and some of a fine purple. Change of colour,

Their weight, which was at first 10 grs. was now found to be Weight after the second heating.

Oak ....7·16	Cherry ....8·60
Elm ....8·18	Linden ....7·86
Beech ....8·59	——— (after having been in the open air twenty-four hours)....8·06
Maple ....8·41	Male fir.........8·46
Ash ....8·40	Female fir......8.66
Birch ....7·40	
Service ....8·46	

Wishing to know whether the wood might not be reduced to charcoal by continuing the moderate heat of the stove a long time, I took half the linden shavings, which weighed 4·03 gr.; placed them in a china saucer, supported by a cylindrical earthen vessel three inches in diameter, and four inches high; put this on an earthen plate, and covered it by a glass jar, six inches in diameter, and eight inches high. On the earthen plate was a layer of ashes, about an inch deep, serving to close the mouth of the jar slightly. Attempt to char a portion by a moderate heat.

This little apparatus being placed in the stove, it was heated a third time for twelve hours; and then left twelve hours without fire, to cool gradually.

On taking out the apparatus I found, that the wood was become perfectly black; and that the glass jar was yellowish, and its transparency diminished. Results.

On weighing the shavings, which retained their original figure completely, I was surprised to find, that they weighed only 2·21 grs. As they were the remains of 5 gr. of wood; and as, from the experiments of Messrs Gay-Lussac and Thenard, I had expected to find in this wood at least fifty per cent of charcoal; I did not think it possible, to reduce the weight of the shavings to less than 2·5 gr., particularly with the moderate heat I employed. Loss of weight.

To

Heated a fourth time.

To clear up my doubts, I replaced the apparatus in the stove, and heated it again as before for twelve hours, and afterward left it in the stove twelve hours to cool.

Results.

On taking out the apparatus I fonnd, that the shavings weighed only 1·5 gr. The jar was less transparent, and of a blackish yellow colour throughout; but particularly in its upper part, above the level of the brim of the saucer, in which the shavings were. These shavings were still of a perfect black.

Heated a fifth time.

Having heated the apparatus again for twelve hours, and then left it to cool, I was surprised on taking it out of the stove the next day to find, that the jar had again become clear and transparent. Not the least trace of the yellow coating, with which its inner surface had been covered, now remained.

State of the wood.

On examining the wood I found, that this also had changed its colour. It had assumed a blueish hue, pretty deep, but very different from the decided black it had before. Its weight was 1·02 gr.

Heated twice more.

I put it twice more into the stove, and each time its weight was diminished, so that the 5 gr. of wood were reduced at last to 0·27 of a gr., or about a twentieth of the original weight.

Charcoal may be dissipated by a heat below combustion.

I am persuaded, that I should have diminished it still more, if I had continued the experiment longer: but it has been tried long enough to establish this remarkable fact, *that charcoal can be dissipated by a heat much less than has been considered necessary to burn it.*

Experiment with common charcoal.

It may be supposed, that I was very desirous of knowing whether the same thing would occur to charcoal already formed by the usual process. Accordingly I took a piece of charcoal from my kitchen, heated it to a strong red heat, and, while it was still red, put it into a marble mortar, and powdered it. Having passed it through a sieve, I took 4·03 gr. of the powder, placed it in the saucer, heated it in the stove twelve hours, and then left it twelve hours to cool. On taking it out it weighed but 3·81 gr.

A second experiment.

As this powdered charcoal was nothing but a collection of small bits of charcoal, which were in contact with the air only by a very small surface compared with that of the

hsavings,

shavings, I made another experiment, the result of which was more striking and more satisfactory.

Having enclosed in a cloth a quantity of powdered charcoal, that had been passed through a sieve, I beat it strongly in a place where the air was still; and when the air appeared to be well loaded with the fine dust of the charcoal, I placed on the ground a white china saucer, quitted the place, and left the dust to settle.

Charcoal in very fine powder employed.

The saucer was covered with it, so as to appear of a very dark gray.

Before all the dust had settled, I wrote some letters on the saucer with the point of my finger, and these letters were afterward covered with a still finer dust.

I imagined it possible, that the part covered by a very fine dust might be found whitened, while that covered with a stratum of coarser charcoal powder would be found perhaps still black.

The result of the experiment showed, that this precaution was not necessary. All the charcoal powder disappeared completely in the stove, and the saucer came out perfectly white.

The whole disappeared in a low heat,

Another saucer, which had been blackened a little by rubbing it with lampblack, and placed in the stove by the side of that blackened with charcoal dust, came out of the stove as black as it went in. As soon as I saw, that the linden shavings converted into charcoal might be dissipated by the moderate heat of the stove, I suspected, that they had been consumed slowly by a silent and invisible combustion; and that the product of this combustion could be nothing but carbonic acid gas.

at which lampblack did not.

To clear up this point I made the following experiment.

Having procured a stock of very dry birch shavings, in ribands about a twentieth of a line thick, near half an inch broad, and six inches long, I dried them for eight days in a room heated by a stove, where the temperature was about 60° F.; the shavings being laid on a table, at a distance from the stove. Of these shavings thus dried, I took 10 gr., which I placed on a china plate, and heated in the stove, in the manner already described, for 24 hours. When taken out of the stove, they weighed but 7·7gr., and had acquired a deep brown colour inclining to purple. They were still wood however,

Experiment to ascertain whether it were converted into carbonic acid gas.

however, for, though deeply browned, they burned with a very fine flame.

Of these brown shavings I made three parcels, each weighing 2·3 gr. The first was placed in the stove on a white china plate, supported by a tile, but not covered. The second was put into it in a similar manner, except that it was covered with a glass jar, six inches in diameter, and six inches high.

The third parcel was put into a glass vessel, six inches high, but only an inch and a quarter in diameter. This narrow vessel was put into a glass three inches in diameter, and seven inches high; which, being slightly closed with its glass cover, was also placed in the stove on a tile.

As the door of the stove (which is double, the better to confine the heat) does not shut so close as to prevent the free passage of air; and as the china plates, on which two of the parcels were placed, were flat; every circumstance was favourable for the free transmission of the carbonic acid gas arising from the decomposition of these two parcels by slow combustion, and there was nothing to prevent the progress of this operation. But the third parcel being enclosed in a narrow vessel, as this gas is much heavier than atmospheric air, the first portion of this gas arising from a commencement of combustion of the wood could not fail to descend in the vessel toward its bottom, gradually expel the air, and at length fill the vessel completely: and as this sort of inundation by carbonic acid gas could not fail to stop the combustion, I expected to find that this parcel of shavings would be preserved, at least in part, even though both the others should be entirely consumed.

Results.

The stove having been heated in the usual manner, I found the next day, that the results of the experiment had been such as I anticipated. The two parcels of shavings placed on the china plates had disappeared entirely; nothing at all remaining, except a very small quantity of ashes, of a white colour inclining a little to yellow.

The yellow ashes in the plate that was not covered with a glass jar were deranged and dispersed by the wind, occasioned by opening the door of the stove too suddenly: but those in the other plate, being protected by the glass, were found all together,

together. As they still retained their original figure of shavings, though reduced to a very small bulk, this appeared to me a demonstrative proof, that the shavings, whence they arose, had not been burned by a common fire. For this reason, and also on account of their extraordinary colour, approaching very near that of the wood in its natural state, I preserved them, to show them to the class. They weighed only 0·04 of a gr.; and as the shavings, of which they were the remains, weighed 2·987 gr. on coming out of the hands of the joiner, these ashes make only one and one third per cent of the weight of the wood.

The third parcel of shavings, which had been placed in a narrow glass vessel, had not disappeared, but the wood was converted into perfect charcoal. I have the honour to present it to the class, in the same vessel in which it was charred.

Reasonings on them.

As the three parcels of shavings were of the same wood, and equal in weight; as they were exposed together to the same degree of heat, and for the same time; and as the two portions, that were placed so as to facilitate the escape of the carbonic acid gas arising from their decomposition, disappeared entirely; while the third, which was so circumstanced that the escape of this gas was impossible, did not disappear: it seems to me, that there can be no doubt of the cause of the phenomena that presented themselves: and it is certainly a curious fact, that charcoal, which has hitherto been considered as one of the most fixed substances known, can unite itself to oxigen, and form with it carbonic acid gas, at a temperature much below that, at which it burns visibly.

Charcoal less fixed than usually supposed.

IV.

Inquiries concerning the Heat developed in Combustion, with a Description of a new Calorimeter; by the Same.*

Results of experiments on heat from combustion disputable.

ATTEMPTS have been long ago made to measure the heat, that is developed in the combustion of inflammable substances;

* Read at the meeting of the fifth class of the French Institute, Feb. the 24th, 1812.

substances; but the results of the experiments have been so contradictory, and the methods employed so little calculated to inspire confidence, that the undertaking is justly considered as very little advanced.

Unsuccessful attempts of the author.

I had attempted it at three different times within these twenty years, but without success. After having made a great number of experiments with the most scrupulous care, with apparatus on which I had long reflected, and afterward caused to be executed by skilful workmen, I had found nothing however that appeared to me sufficiently decisive to deserve to be made public. A large apparatus in copper more than twelve feet long, which I had made at Munich fifteen years ago; and another scarcely less expensive made at Paris four years ago, which I have still in my laboratory; attest the desire I have long entertained of finding the means of elucidating a question, that has always appeared to me of great importance, both with regard to the sciences, and to the arts.

Simple and accurate method discovered.

At length, however, I have the satisfaction of announcing to the class, that, after all my fruitless attempts, I have discovered a very simple method of measuring the heat manifested in combustion, and this even with such precision, as leaves nothing to be desired.

That the class may be the better able to judge of my method of operating, and the reliance that may be placed on the results of my experiments, I place my apparatus before it.

The apparatus described.

The principal part of this apparatus is a kind of prismatic receiver, eight inches long, four inches and a half broad, and four inches three quarters high*, formed of very thin sheets of copper. This receiver, which well deserves the name, already celebrated, of *calorimeter*, is furnished with a long neck, near one of its extremities, three quarters of an inch in diameter, and three inches high, intended to receive and support a mercurial thermometer of a particular shape. The receiver has also another neck, an inch in diameter and the same in height, situate in the centre of its upper part, and closed by a cork.

A calorimeter.

Worm of a new form.

Within this receiver, two lines above its flat bottom, is a particular kind of worm, receiving all the products of the combustion

* French measure.

combustion of the inflamable substances burned in the experiments; and transmitting the heat manifested in this combustion to a considerable body of water, which is in the receiver.

This worm, which is made of thin copper, occupies and covers the whole bottom of the receiver, yet without touching either its bottom or its sides. It is a flat tube, an inch and half broad at one end, and an inch at the other; and half an inch thick throughout. It is bent horizontally, so as to pass three times from one end of the receiver to the other; and is supported in its place, two lines above the bottom of the receiver, by several little feet.

The aperture, that forms the mouth of the worm, is a circular hole in its bottom, near its broadest end. Into this hole is soldered a perpendicular tube, an inch in length and an inch in diameter, reaching within the worm to the height of a quarter of an inch above its bottom.

This tube passes through a circular hole in the bottom of the receiver, to which also it is soldered. Its lower aperture is seven lines below the bottom of the receiver; and through this the products of the combustion enter into the worm.

The other extremity of the worm passes horizontally through the perpendicular end of the receiver, opposite to that near which the products of the combustion enter the worm.

The worm, before it passes through the end of the receiver, is fashioned into the shape of a round pipe, half an inch in diameter; and an inch in length of this pipe is seen without the receiver. This piece is made to fit tight into another similar tube, belonging to the worm of another receiver, which I call the *secondary receiver*; the purpose of which is to receive the heat, that might still be found in the products of combustion, after they have passed through the worm of the principal receiver.

Secondary receiver.

To support these two receivers in the air, so as not to touch the table that supports them, each of them is fixed in a frame of dry linden wood, made of rods an inch square. Round the bottom of each receiver is a copper rim, three lines deep, which is fastened by a row of very small nails to the wooden frame. The body of the receiver itself enters about a line into the frame, to which it is very accurately fitted.

Mode of supporting them.

The

Flatness of the worm essential.

The flat form of the worm is essential to the perfection of the apparatus; as is evident, when its purpose is considered.

All the products of the combustion being elastic fluids, and consequently substances incapable of communicating their heat, but by proceeding particle after particle to deposit it on the suface of the cold and fixed body intended to receive it, it was indispensable so to construct the apparatus, that the hot fluids should of necessity be spread *beneath* and *against* a large flat surface, placed horizontally, and always cold.

This shape advantageous for a common still.

Before I employed horizontal worms made of flat tubes, I had more than once tried those of the common form: but they never answered my purpose otherwise than so imperfectly, that I could never make any account of the experiments, in which they were employed. There is no doubt but the shape I have adopted for the worm of my calorimeter would be very advantageous for every kind of apparatus for distillation.

Shape of the thermometer.

One thing very important in the construction of my apparatus is the shape of the thermometer, which I employ to measure the temperature of the water in the receiver. This thermometer, which I made myself; and which, after having undergone every kind of trial, has always appeared good; is a mercurial thermometer, divided according to Fahrenheit's scale. It is one of four, all similar, that I employed, at Munich, in the winter of 1802, in my ex-experiments on the refrigeration of liquids enclosed in vessels.

The reservoir of this thermometer is cylindrical, about two lines in diameter only, and four inches high: and as the water in my calorimeter is four inches deep, this thermometer always indicates the mean temperature of the fluid, whatever may be the temperature of its different strata.

To measure the heat of a fluid the bulb of the thermometer should extend from bottom to top.

In my various inquiries concerning heat, I have had frequent opportunities of seeing the importance of this precaution; and I cannot conceive how any one can expect to avoid great mistakes in measuring the temperature of liquids heated or cooled, if we do not attend to this.

this. For my own part, I confess, I pay little regard to the experiments of which I am told, when I know they are so negligently made; and assuredly I shall never waste my time, in attempting to build theories on their results.

Complete combustion requisite.

In using the apparatus I have described, several precautions are necessary. In the first place it is obvious, that, when the object is to ascertain the quantity of heat developed in the combustion of any inflammable substance, it is indispensably necessary, so to arrange matters that *the combustion shall be complete.* I have thought, that it might be so considered, whenever the substance burned leaves no residuum, and burns with a clear flame, without smoke or smell.

Smell of the burning substance a proof it is imperfect.

The least smell, particularly that peculiar to the inflammable substance bnrned, is a certain indication, that the combustion is imperfect.

I had long sought, before I was able to find to my satisfaction, a mode of burning very volatile liquids, such as alcohol and ether: but I have at length discovered it, as will soon appear. I have frequently succeeded in burning highly rectified sulphuric ether, without the least smell of ether being diffused through the room; and it was in these instances alone, that I considered the experiments as accurate.

Method of burning wood completely.

As to wood I have found a very simple method of burning it completely, without the least appearance of smoke or smell. I got a joiner to plane me shavings about half an inch wide, a tenth of a line thick, and six inches long: and holding these in the hand or with pliers, elevated at an angle of 45° or thereabout, and with the edges perpendicular, they burned like a match, with a very clear flame.

The slip of wood that burns being very thin, and placed between two flat flames, which press on it closely, it is exposed to the action of so strong a heat, that it burns perfectly and entirely.

If the shavings employed be too thick, a portion of the charcoal of the wood remains; particularly if it be oak, or any other wood of slow and difficult combustion: and in this case the experiments are defective. But if the shavings

be

be sufficiently thin, and well dried, I have found, that any kind of wood may be burned completely.

Management of candles and lamps.

In burning candles, wax tapers, or fat oils in lamps, the only precautions necessary are so to arrange the wick, as to yield no smoke; to place the flame properly in the aperture of the worm; and to surround the apparatus on all sides by screens, to prevent the flame from being deranged by the wind.

Source of errour from the air cooling the receiver,

In these experiments there is one source of errour, too obvious to escape the most superficial observer, and to which it was important to attend. While the calorimeter is warmed by the heat developed in the combustion of the inflammable substance, which is burning at the aperture of the worm, it is continually cooled by the ambient air, that surrounds it on all sides. It would be possible, no doubt, by calculations founded on a knowledge of the law of refrigeration of the receiver, which might be found by separate experiments, to ascertain the quantity of the effect produced by the refrigeration in question; and this even with a certain degree of precision: but it would have been impossible by this method, or by any other known, to calculate the effects of another cause of errour, less obvious perhaps, but certainly more weighty, than that of the refrigeration of the external surface of the receiver.

and from the nitrogen carried into it.

The nitrogen, which is mixed with the oxigen of the atmospheric air, is necessarily carried into the worm with the proper products of the combustion; and without a precaution, which it occurred to me to employ to prevent the effects of this cause of errour, by making a compensation for them, all the experiments would have been of no value.

Fortunately the method I employed to obviate the effects of this cause of errour was sufficient, to prevent at the same time those, that might have arisen from the cooling of the outer surface of the receiver.

Method of obviating both.

As the receiver is cooled, whether by the atmospheric air in contact with its external surface, or by the nitrogen and other gasses traversing the worm with the products of combustion, only so far as the worm is hotter than the surrounding

surrounding air; while on the contrary it is heated by these elastic fluids, whenever it is at a lower temperature than they are: by arranging matters so, that the temperature of the water in the receiver shall be a certain number of degrees, 5° for instance, below the temperature of the air at the beginning of the experiment; and putting an end to the experiment, as soon as the water in the receiver has acquired a temperature precisely the same number of degrees higher than the air; the receiver will be heated by the air during half the time of continuance of the experiment, and cooled by it during the other half: so that the calorific and frigorific effects of the air on the apparatus will counterbalance each other, and produce no perceptible effect on the results of the experiments; consequently they will require no correction.

When we are making experiments to elucidate natural phenomena, it is always more satisfactory to avoid errours, or to compensate them, than to trust to calculation for appreciating their effects. Better to avoid or compensate errour, than correct it by calculation.

As the law of the variation of the specific heat of water at different temperatures is not known, and as we have but an imperfect knowledge of the true measure of the intervals of temperature marked by the divisions of our thermometers, to prevent the effects, that our uncertainty on these points would have on the subject of inquiry, I took care to make my experiments in a room, where the temperature varied very little, and to confine them to a few degrees of elevation of the temperature of the water in the receiver. A small range of the thermometer used.

It is true, I made some experiments in a room where the air was much colder, and in which I employed ice instead of water to fill the receiver; but these experiments were for a particular purpose, and are not classed with the others. Besides, they never afforded such uniform and satisfactory results, as those made under other circumstances. Other experiments.

It has been fully proved, not only by the results of my experiments, but by the experiments of others also, that the vapour of water in contact with ice frequently freezes, while this same ice is melting by the heat, or that its thaw appears fully established. Freezing of aqueous vapour

To

Experiment to ascertain the perfection of the apparatus.

To give an idea of the reliance that may be placed on the results of the experiments made with the new apparatus I have just described, I will introduce here the particulars of an experiment, made purposely to discover its degree of perfection.

Having filled two receivers, properly connected with each other, with water at the temperature of the air of the room, 55° F., I burned a wax taper under the mouth of the principal receiver, so that all the products of the combustion passed through the worm of the secondary receiver, after having traversed that of the principal. Each of the receivers contained 2371 gr. [36621·5 grs.] of water.

The following are the results of the experiment.

Time of the observation.			Temperature of the water	
Hours	Min.	Sec.	in the principal receiver.	in the secondary receiver.
9	37		55°	55°
	49	42	65	55
	56	15	70	55
10	2	52	75	$55\frac{1}{8}$
	9	32	80	$55\frac{3}{8}$
	16	34	85	$55\frac{5}{8}$
	23	54	90	$55\frac{3}{4}$
	27			56
	31	40	95	$56\frac{1}{8}$
	39	35	100	$56\frac{3}{8}$
	47	40	105	$56\frac{3}{4}$

The secondary receiver not used in the following experiments as unnecessary.

From the results of this experiment it appears, that the water in the secondary receiver did not begin to be heated perceptibly, till that in the principal receiver had been heated 15° or 20°: and, as I had intended from the beginning never to continue an experiment longer than was necessary to raise the temperature of the water in the principal receiver 10° or 12° F.; it may be supposed, that, as soon as I found by this experiment how little heat remained in the products of combustion after they had passed through the worm of the principal receiver, I relinquished my original design of operating with the two receivers joined together. As it was evident, from the above results, that the second receiver could never be

be sensibly affected; or indicate any thing except the confidence I might place in the indications of the first, I resolved to dispense with the trouble of using it.

It may be seen by the description I have given of this apparatus, that it may be used very conveniently for ascertaining the specific heat of gasses; as well as that made apparent in the condensation of vapours; and generally in all researches, where the quantity of heat communicated by an elastic fluid in cooling is to be measured. And as it would be extremely easy, by very simple means, to separate completely the products of the vapours condensed in the worm from the gasses, that pass through it without being condensed, I cannot avoid hoping, that this apparatus will become useful as an instrument to be employed in chemical analyses. This however would only be an extension of the method already employed with so much success by Mr. de Saussure, and by Messrs. Gay-Lussac and Thenard. The apparatus applicable to ascertain the specific heat of gasses.

As soon as my apparatus was finished, I was eager to see what quantity of heat I should find in the combustion of wax, and in that of olive oil, that I might afterward compare the results of my experiments with those of Mr. Lavoisier's: and, as I have the most implicit reliance on every thing published by that excellent man, I sincerely wished to find in this comparison a proof of the accuracy of my method, and at the same time a confirmation of the estimates of Mr. Lavoisier. Experiments to be compared with Lavoisier's.

SECT. I. *Experiments made with white wax.*

The air of the room being at the temperature of 61° F., 2781 grammes of water, of the temperature of 56° F., were put into the receiver of the calorimeter, (including the quantity of this liquor that represents the specific heat of the instrument); and, a lighted wax taper having been properly placed at the entrance of the worm, the calorimeter was heated for 13 min. 26 sec.; when, the thermometer announcing that the water had acquired the temperature of 66° F., the taper was extinguished. Combustion of white wax.

As I took care to weigh the taper before it was lighted, I found by weighing it at the end of the experiment, that 1·63gr. of wax had been burned.

To express the results of this experiment so as to render them obvious, and at the same time easy to be compared with the results of other similar experiments, we will see how much water of the temperature of melting ice would have been made to boil, at the mean pressure of the atmosphere, by the heat made apparent in the combustion of the 1·63gr. of wax burned.

The distance on Fahrenheit's scale between the temperature of melting ice and boiling water being 180°, if the burning of 1·63gr. of wax were requisite to raise the temperature of the water in the calorimeter 10°, the burning of 29·34gr. would have been necessary, to raise it 180°: and, if 29·34gr. of wax could furnish by combustion sufficient heat to raise the temperature of 2781gr. 180°, a gramme of this inflammable substance must furnish enough, to heat 94·785gr. of water to the same point.

Quantity of water heated 180° by it.

Consequently one pound of white wax, or wax taper, should furnish in burning sufficient heat, to raise 94·785lbs. of water from the temperature of melting ice to the boiling point.

Quantity of ice melted.

To find how many pounds of ice this quantity of heat would melt, we have only to add to the number of pounds of water at the temperature of melting ice it would cause to boil the third part of this number, and the sum would express the weight of the ice in pounds.

This, then, for white wax is······94·785
+ 31·595

= 126.380lbs. of ice melted for one pound of the wax burned.

Two other experiments with wax.

Before I compare the result of this experiment with that of an experiment made with the same substance by Mr. Lavoisier, I will give an account of two other experiments I made with wax, as the reader will undoubtedly be struck with the uniformity of their results. This is so remarkable, that I should scarcely venture to publish them, had I not proofs, that all my experiments were actually made and minuted down, before I began my calculation of their results; and were I not assured, that any person, who will follow my method, using the same apparatus, will find the same results on repeating my experiments.

As the mode of operating in making these experiments must now be well known, I may suppress the particulars in what follows without inconvenience, and give only the results of the experiments.

I will begin with three experiments made with white wax; and to render them more easy to compare, I will give them together in a tabular form.

Tabulated results.

Results of three experiments on the burning of white wax, showing the quantity of water that would be heated 180°, *or of ice that would be melted, by one pound weight of it.*

No. of the expt.	Quantity of wax burned.	Time employed in burning.	Quantity of water heated.	Elevation of its temperature.	Temper. of the water at the beginning.	Temper. of the water at the end of the exp	Temperature of the air.	Results: lbs of water heated 150°	Results: lbs of ice melted.
	Grammes.	Min. Sec.	Grammes.	Deg. Fahr.	Deg. Fahr.	Deg. Fahr.	Deg. Fahr.	lbs.	lbs.
1	1·63	13 24	2781	10°	56°	66°	61°	94·785	126·38
2	2·36	19 30		14·5	51	65°5	58	94·926	126·608
3	2·17	18 15		13·25	51·75	65	58	94·337	125·783

If

Mean of the three experiments.

If we take the mean term between the results of these experiments, we shall find, that the quantity of heat developed in the combustion of wax is such, that one pound of this substance is sufficient, to raise 94·682lbs of water from the temperature of melting ice to the boiling point; and consequently, that it should melt 126·242lbs of ice.

Results of Lavoisier's.

According to the experiments of Mr. Lavoisier, the heat developed in the combustion of one pound of white wax was sufficient to melt 133·166lbs of ice.

The difference small:

The difference between the results of our experiments with this substance is not very great; and, if those of Mr. Lavoisier were made at a time, when the temperature of the air was only a few degrees higher than that of melting ice (which I have no means of ascertaining), the quantity of nitrogen, that must have entered into the calorimeter with the oxigen employed to support the combustion, would have been so great as to account sufficiently for the difference. But the very great difference between the results of our experiments made with olive oil proves, that one or other of our processes must have been defective.

but great in combustion of oil.

Result of the author's experiments:

The mean result of several experiments made with olive oil gave me for the measure of the quantity of heat developed in the combustion of one pound of this substance 90·439lbs of water heated 180° F; or 120lbs of ice melted, neglecting the fraction.

and of Lavoisier's.

In the experiments of Mr. Lavoisier more than 148lbs of ice were melted by the heat, that appeared to result from the combustion of one pound of this oil.

The latter suspected to be great by himself.

It is true, that this result was considered by that eminent philosopher himself as too great to be capable of explanation; and he added, with that modesty which rendered him so engaging and so respectable: "We shall probably find ourselves under the necessity of making corrections, perhaps pretty considerable ones, in most of the results I have given: but I did not think this a sufficient reason, to delay affording their assistance to those, who might intend to pursue the same object."

Rape oil purified by sulphuric acid compared with olive oil.

As it appears very probable, that all the fat oils, when perfectly pure, are composed of the same principles, I was curious to see whether rape oil, purified by sulphuric acid, would not

not afford more heat in its combustion than olive oil, when burned in its natural state. The result of three experiments showed me, that rape oil thus purified does in fact yield more heat than olive oil. The difference is indeed pretty considerable, and more than I could have suspected.

The combust. of 1lb of purified rape oil gave
93·073 of water heated 180°.
———— olive oil gave 90·439.

Chemists may tell us, whether the quantity of incombustible matter separated from rape oil in purifying it be sufficient, or not, to account for this difference.

Comparison of oil and wax.

On comparing the results of the experiments made with white wax and those with the purified oil, it appears, that equal weights of these substances afford nearly equal quantities of heat in their combustion: and as in fact this ought to be the case, from the quantities of combustible matter they contain, the result tends to strengthen our confidence in this method of measuring the heat developed in combustion.

It was with the combustion of
1lb of white wax 94·682lbs of water heated 180°.
1lb of purified oil 93·073lbs.

Combustion of hidrogen and carbon the object of research.

As the object I had chiefly in view in this series of experiments was to ascertain the quantities of heat developed in the combustion of pure hidrogen and carbon, in order to render this method useful in some chemical analyses, I examined particularly those inflammable substances, that had been analysed with most care.

This has been attempted directly.

Several attempts have been made to ascertain these interesting questions by direct experiments, in burning pure hidrogen, or pure hidrogen and carbon; but the results of these researches have varied so much, that they cannot be relied on.

Hidrogen rated higher by Crawford than by Lavoisier:

According to Crawford, the heat developed in the combustion of one pound of hidrogen gas is sufficient to raise the temperature of 410lbs. of water 180° F. But the estimation of Mr. Lavoisier is much lower. According to him this heat would raise only 221·69 lbs of water the same number of degrees.

On

carbon the contrary.

On the other hand Mr. Lavoisier estimates the quantity of heat developed in the combustion of charcoal much higher than Dr. Crawford. I have many reasons to believe, that they both estimate it too high: and, if this opinion be confirmed, we must estimate the heat developed in the combustion of hidrogen a little higher even than Crawford has done, to be able to account for that manifested in my experiments.

Perhaps both rate this too high, hidrogen too low.

Charcoal according to the author,

From several experiments, which I made five years ago, it appeared to me, that one pound of charcoal, dried as much as possible before it was weighed by heating it red hot in a crucible, was not capable of raising more than from 52 to 54 lbs. of water from the temperature of melting ice to a boiling heat.

Crawford, and Lavoisier.

According to Crawford this heat should suffice to boil 57·606 lbs.; and according to Lavoisier, 72·375 lbs.

We shall see how these estimates agree with the results of my experiments.

Results, from wax compared with those calculated from its component parts:

As the experiments made with wax yielded very uniform results, and as the analysis of this substance has been made with great care, I shall examine how the quantities of hidrogen and carbon in this substance agree with the quantity of heat, that it afforded me in combustion.

According to the analysis of Messrs. Gay-Lussac and Thenard, a pound of this substance contain

Carbon	0·8179
Free hidrogen	0·1191

If we adopt the calculations of Dr. Crawford, both for the heat furnished by the hidrogen, and that furnished by the carbon, we shall have for the heat that should be furnished by the combustion

according to Crawford,

	lbs of water raised from 32° to 212°
Of 0·1191 lb. of hidrogen, after the ratio of 410 lbs. of water raised from 32° to 212° by burning 1 lb. of hidrogen	48·831
Of 0·8179 lb of carbon, after the ratio of 57·606 lbs. of water raised from 32° to 212° by burning 1 lb. of carbon	47·116
Total of the heat, that ought to be furnished by the quantity of combustible matter (hidrogen and carbon) in 1 lb of white wax	95·947 lbs

Quantity

Quantity of heat furnished by 1 lb. of white wax, during its combustion, according to my experiments .... 94·682 lbs

to the author's experiments,

If we adopt the calculations of Mr. Lavoisier for the heat furnished by carbon and hidrogen in their combustion, we shall have for the heat that ought to be furnished by the burning

to Lavoisier.

Of 0·8179 lb. of carbon, after the ratio of 72·375 lbs. of water heated 180° by 1 lb..... 59·195 lbs
Of 0·1191 lb. of hidrogen, after the ratio of 221·69 lbs. of water heated 180° by 1 lb. .... 26·403
Total of the heat that ought to be furnished by the combustible matter in 1 lb. of white wax .... 85·598 lbs

From the results of these calculations it appears, that the estimations of Dr. Crawford agree much better with the experiments than those of Mr. Lavoisier.

Crawford's nearer than Lavoisier's.

Let us see how the results of the experiments made with fat oils agree with the estimations of these gentlemen.

Comparison of fat oils.

According to the analysis of Messrs. Gay-Lussac and Thenard a pound of olive oil contains

Carbon ....0·7721 lb
Free hidrogen ....0·1208

Results,

According to the calculations of Mr. Lavoisier we have,

according to Lavoisier,

For 0·7721 lb. of carbon ..55·881 lbs. of water heated 180°
0·1208 lb. of hidrogen 26·780

Total 82·661

According to the calculations of Dr. Crawford it is

to Crawford,

For 0·7721 lb. of carbon ..44·478 lbs. of water heated 180°
0·1208 lb. of hidrogen 49·528

Total 94·006

According to the experiments 1 lb. of purified rape oil furnished heat sufficient to raise 93·073 lbs. of water 180°; and 1 lb. of olive oil enough to heat 90·439 lbs.

to experiment.

From all these comparisons it follows, that the estimations of Dr. Crawford agree much better than those of Mr. Lavoisier with the results of my experiments.

Crawford's still nearest.

SECT.

SECT. II. *Experiments made with spirit of wine, alcohol, and sulphuric ether.*

Experiments with inflammable liquids.

As the component parts of these inflammable liquids may be considered as well ascertained by the results of the excellent investigation of Mr. de Saussure*, I undertook to examine them for the second time, in order to discover what quantities of heat are developed in their combustion. I had begun this undertaking five years ago; but, after having made a considerable number of experiments, I desisted from it, on account of the great difficulties that occurred. As soon, however, as I had found means of rendering my apparatus more perfect, I formed the project of recommencing it.

Difficulties in them,

Before I enter into the particulars of my experiments, I must say a few words respecting the difficulties that occurred to me, even after I had my new apparatus; and of the means I employed to surmount them. I even found myself exposed to dangers, which it is necessary for me to mention as a caution to those, who may undertake the same inquiry.

and even danger.

Evaporation of the liquid.

When I made the experiments with highly rectified alcohol, and more particularly with ether, I found it very difficult to prevent a portion of these volatile liquids from escaping in the state of vapour from the bulk of them remaining in the lamp. I procured a small lamp, resembling in shape a small round snuffbox, with a nozzle rising from the centre of the circular plate, which closed it atop; and on this plate was fixed a small pan, to hold cold water, for keeping the nozzle cool, and preventing the heat from being communicated to the body of the lamp. But this precaution was not sufficient, when I burned ether, as I found to my cost: for though the pan was twice the diameter of the lamp, and filled with very cold water, the water was so heated in a few minutes, that an explosion took place from vapour of ether kindling in the air with a flame that rose to the ceiling. Indeed it was near setting the house on fire.

Attempt to prevent it.

Dangerous explosion.

Another lamp constructed.

Warned by this accident I procured a new lamp, much smaller than the former, being only an inch in diameter and and three quarters of an inch deep; and its nozzle, which was only two lines in diameter, was three quarters of an inch

* See Journal, vol. xxi, pgs. 222, 259, 321.

high.

high. To keep this small lamp cool while burning, it was placed in a small pan, and kept constantly immersed in a mixture of water and pounded ice to within a quarter of an inch of the extremity of the nozzle. These precautions were sufficient to prevent any explosion, though not the evaporation either of the ether or of the alcohol. This fact I learned from observing, that, as often as I made two consecutive experiments without filling the lamp afresh, the alcohol constantly appeared weaker in the second experiment than in the first.

This prevented explosion, but not evaporation.

The cause of this phenomenon was not difficult to discover. The most volatile, and consequently the most combustible parts of this liquid, being diffused in vapour in the interior of the lamp, found means of escaping through the nozzle with the part of the liquid that traversed the match, leaving the alcohol, that remained in the lamp, perceptibly weakened.

To remedy this imperfection I constructed a third lamp, which I now submit to the inspection of the class. It is made of copper, and has the shape of a small cylindrical vase, an inch and half in diameter, and three inches high, swelling out a little atop, and closed hermetically by a copper stopple, which, being ground with emery, fits tight into the neck of the vase. Through the centre of this stopple passes a small perpendicular hole, which can be shut completely, or left a little open, as may be required, by means of a small screw carrying a copper collar.

A third lamp that succeeded completely.

A small tube, about an eighth of an inch in diameter and two inches and half long, proceeds horizontally from the side of the vase very near the bottom. At the distance of an inch and four lines from the vase this tube is bent at a right angle, rising upwards perpendicularly to form the nozzle of the lamp.

This little tube is every where very thin, except at its upper extremity, where it is made thicker, to admit of being shaped so as to fit tight into a very small cylindrical extinguisher, 5 lines high by 3·5 in diameter; intended to close the nozzle hermetically without touching or deranging the wick, the moment the lamp ceases to burn; and to keep it constantly closed, when the lamp is not lighted.

Without

Without this precaution; in experiments made with ether, so large a quantity of this volatile liquid would evaporate through the nozzle of the lamp while weighing, that it would be impossible to ascertain the quantity burned.

The nozzle of the lamp is steadied by two pieces of wire, proceeding from it horizontally, and soldered to the body of the lamp.

To keep this lamp constantly cold, as well as the liquid it contains, it is placed in a small pan, and covered completely, except the extremity of its nozzle and that of its neck, with a mixture of pounded ice and water.

Caution.

When the lamp is weighed, it is taken out of the pan, and well wiped with a dry cloth, before it is put into the scale.

When the lamp is kindled, the operator must not forget, after it has burned two or three minutes, to open the screw that closes its stopple a little, though but *very little*, otherwise it might go out.

As the little horizontal tube, by which the liquid that is burned passes from the reservoir of the lamp to its nozzle, is always filled with liquid, so that it can have no communication with the vapour diffused in the upper part of the reservoir, this vapour cannot escape by the nozzle of the lamp, as it did before I thought of this method of preventing it.

Apology for minuteness.

If I have been very minute in my description of this lamp, it was because I thought it necessary to spare those, who might be disposed to repeat my experiments or make similar ones, all the difficulties I had to surmount, before I found the means of having under command the combustion of very volatile inflammable liquids.

As the apparatus I have employed has now been described, it will be easy to follow the steps of my experiments, and to appreciate their results. I will endeavour to describe them clearly, but also as briefly as possible.

Spirit employed in the experiments.

Having procured a stock of spirit of wine of the shops, and of alcohol of different degrees of purity, I ascertained with the greatest care their specific gravities at the temperature of 60° F., taking that of water at the same temperature as 100000. I chose this temperature, that I might afterward the more easily ascertain the quantities of water, that each ought to contain, according to the tables constructed from the experiments of Mr. Lowitz.

The following table will show the specific gravity of each, and the quantity of pure alcohol of Lowitz and of water contained in it.

Liquid.	Spec. grav. at 60° F.	Composition. Pure alcohol of Lowitz.	Water.
Alcohol of 42°	817624	0·9179	0·0821
Alcohol of the shops	847140	0·8057	0·1943
Spirit of wine of 33°	853240	0·7788	0·2212

The following are the results of the experiments made to ascertain the quantities of heat, which these liquids furnished in burning. Results

In three experiments made with the spirit of wine the quantities of heat manifested were, with the weakest spirit:

$$\left.\begin{array}{r}\text{in the 1st, } 53{\cdot}260 \\ \text{2d, } 51{\cdot}727 \\ \text{3d, } 52{\cdot}604\end{array}\right\} \text{lbs of water raised from the temperature of melting ice to that of ebullition.}$$

The mean result is 52·604 lbs*.

As a pound of this liquid contained but 0·7788 of the alcohol considered by Lowitz as pure; the other part, = 0·2212, being only water, which does not burn; to find how much water would be raised from the temperature of melting ice to that of ebullition by a pound of the pure alcohol of Lowitz, we have only to divide the quantity, that is the measure of the mean heat developed in the experiments with the spirit of wine by the fraction, that expresses the quantity of alcohol in a pound of this liquid.

Thus we have $\frac{52{\cdot}604}{0{\cdot}7788} = 67{\cdot}545$ lbs, the measure of the heat developed in the combustion of one pound of pure alcohol of Lowitz, according to the mean result of the experiments made with spirit of wine.

In two experiments made with the alcohol of the shops, the mean result was 54·218 lbs: and, as this contained with the next:

* As the mean of the three preceding numbers would be 52·530, there is evidently some mistake; and the last number of the three being the same with the mean given, it is probable, one of these is an errour of the transcriber. But, as the number 52·604 is employed as the mean in the calculation in the next paragraph, it may be presumed, that the result of the third experiment should have been 52·825. C.

0·8057lb

0·8057 lb. of pure alcohol, we have for the measure of the heat developed in the combustion of 1lb of pure alcohol $\frac{54·218}{0·8057}$ = 67·294 lbs of water heated 180° F.

with the strongest.

Of three experiments made with the alcohol at 42° the mean result was 61·952 lbs. of water heated 180° F. by the heat developed in the combustion of one pound of this liquid.

Hence 1lb of pure alcohol should furnish heat enough in burning to raise 67·57 lbs of water 180° F.; for $\frac{61·952}{0·9179}$ = 67·57*.

Mean for pure alcohol:

Taking the mean between the results of these eight experiments with three alcoholic liquors, we shall have for the measure of the heat developed in the combustion of one pound of pure alcohol of Lowitz 67·47† lbs of water raised from the temperature of melting ice to that of ebullition.

compared with its component parts.

It will be extremely interesting, no doubt, to know whether this quantity of heat agree with the quantities of combustible matter (carbon and hidrogen) in alcohol. We will see.

According to the analysis of Mr. de Saussure, 1lb of the alcohol of Lowitz contains

Carbon	0·4282
Free hidrogen	0·1018
Water	0·4700
	1·

The calculation from Crawford,

Now according to the calculations of Dr. Crawford we shall have for the measure of the heat developed in the combustion of

0·4282 lb of carbon	24·667 lbs of water heated 180° F.
0·1018 lb of hidrogen ..	41·738
Total	66·405

and the re-

The experiments gave us 67·47†

* If the mean result were as given above, which I have no means of knowing, as the results of the experiments are omitted, this should be 67·493. C.

† If the correction in the preceding note were to be made, this should be 67·444. C.

It

It is rare in a research of such delicacy to find the results of experiment agree so perfectly with those of calculation. Result of the experiment, very nearly agree.

In the conclusion of this paper I shall have the honour of giving the class an account of the results of a considerable number of experiments, which I have just made to ascertain the quantities of heat developed in the combustion of sulphuric ether, naphtha, suet, and several kinds of wood; as well as that manifested in the condensation of the vapours of water, of alcohol, and of ether. Farther experiments with other substances

These experiments are all finished, and I have made considerable progress in the paper, in which I purpose to give an account of them here. I flatter myself, that the class will do me the honour to listen to it with its usual indulgence, at an early meeting*. promised.

V.

Remarks on the Experiment of Dr. Bostock *and Dr.* Traill. *In a Letter from a Correspondent.*

To W. NICHOLSON, Esq.

SIR,

THE experiments of Drs. Bostock and Traill cannot, I think, be considered as decisive in proving, that water is produced in the combination of dry muriatic and ammoniacal gas. The mode adopted in drying the ammoniacal gas by these gentlemen is not effective. A lump of quicklime introduced into a jar of this gas would not absorb the whole of the combined moisture. Lime is not so greedy of moisture as some other substances, and in a mass would be disposed to take up but little: had it been introduced immediately from the fire in the state of powder, it might have been more effectual. To deprive gas of the moisture it contains, the best method has been found to be to pass Experiment of Dr. Bostock and Dr. Traill not decisive, a lump of lime being insufficient to dry muriatic gas. Hot lime in powder better;

* This the Count has promised to transmit for insertion in the Journal, as soon as he can find an opportunity.

it

but hot muriate of lime best.

it repeatedly through muriate of lime in coarse powder previously heated; or to agitate it for some time in contact with this salt in a dry vessel.

If Drs. Bostock and Traill will take the trouble of repeating their experiment with this precaution, they will, I believe, find the result to be as A. B. C. has stated it.

I am, Sir,

Your most humble servant,

BRISTOL, 11th of May, 1812. D. E. F.

VI.

Method of preparing a cheap and durable Stucco, or Plaster, for outside or inside Walls: by H. W. WAY, Esq. *of Bridport Harbour**.

SIR,

Stucco for houses exposed to bad weather.

IN consequence of your expressing an opinion, that a general knowledge of my method of preparing a stucco, or plaster, for outside walls of houses much exposed to sea breezes or bad weather, would be of service to the public, I have enclosed an account of the process; and I will with pleasure furnish any farther particulars of this business for the Society of Arts, or permit any gentleman to examine it, who may wish for more information on the subject. You know the situation of my house, which is greatly exposed to the spray of the sea and bad weather; and I can truly add, that by means of this stucco it is perfectly free from damp, and the plaster remains compact and durable.

I remain, Sir,

Your obedient humble servant.

BRIDPORT HARBOUR, Oct. 12, 1810. H. B. WAY.

* Trans. of the Soc. of Arts, vol. XXIX, p. 73. The silver medal was voted to Mr. Way; and specimens of his stucco, and of the sand from which it was made, are preserved in the Society's repository.

To

To make a strong Stucco, or Mortar.

Three parts Bridport Harbour sand to one of lime, both finely sifted and mixed with lime-water; if used as stucco, the first coat to be laid on half the thickness of a crown-piece; let it remain two days, then with a painter's brush wash it over with strong lime water, and lay on the second coat of the same thickness. Method of making it.

1805, March 25.—Measured a coal half-bushel of Beamister lime*, and put it into a hogshead of water, to make the lime-water.—Measured two coal half-bushels more of the lime, slaked and sifted it, it then measured three half-bushels, to which were added nine coal half-bushels of Bridport Harbour sand well sifted; I saw it well mixed up with lime-water, and thoroughly worked together; the next day saw it turned, and again mixed up, that it might be well incorporated together.

27th.—This morning had a fine coat of it laid on the west end of my large storehouse at Bridport harbour.

29th.—Had it washed with lime-water, and a second coat laid on.

Cost.

	s.	*d.*
One sack and a quarter of lime, at 2*s.* 6*d.* -	3	$1\frac{1}{2}$
Two men and one boy two days each, fetching and mixing up materials, and laying on; men 2*s.* 3*d.* per day, boy 10*d.* per day, and one pint of ale each per day, 12*d.* - - -	11	$10\frac{1}{2}$
	15	0

Expense.

N. B.—I suppose the expense rather over than under-rated.

May 11.—This day Thomas Everett measured and examined the work, found it hard and sound, $24\frac{7}{9}$ square yards, a little done to the house, suppose the whole to be twenty-five yards square.

Twenty-five square yards at $7\frac{1}{4}$*d.* per square yard, would be 15*s.* $1\frac{1}{4}$*d.*

* This appears, from a subsequent part of the paper, to be chalk lime. C.

June

Its durability. June 13, 1806.—Examined the work, which was perfectly sound and free from cracks, nothing having ever peeled off from it. The situation exposed to the weather in the greatest degree.

N. B.—The coal half-bushel above mentioned holds exactly thirteen gallons wine-measure.

H. B. WAY.

SIR,

Farther account of the stucco. I WAS favoured with yours of the 18th instant, and I now enclose the mason's certificate of the quantity of stucco done with the composition I gave him the particulars of; in addition to which it may be necessary to mention, that the coal half-bushel, with which the ingredients of the composition were measured, (according to the account formerly given), contains exactly thirteen gallons of water, wine measure, and would hold exactly 1 cwt. 1 qr. 7 lb. net of the sand used. The weight of the lime I do not know; and my being able to ascertain exactly the weight of the sand arose from my waggon being employed to carry what was used at Yeovil, and East Coker, from hence; and for what I sent to Yeovil I was paid 1*s.* 9*d.* per cwt. From the sand here succeeding so well, Thomas Everett would not be prevailed on to engage to do any of that sort of work with hill or river sand, to be got on this shore. Charge for it. The work he did for me was all by the day; what he did at Yeovil and East Coker he agreed for at eight pence per yard, of nine feet superficial measure for labour only for the two coats, at four pence per square yard for one coat, all the materials being brought to the spot at his employer's expense, and who also found scaffolding and scaffold ropes. This, I think, is considerably higher than by my calculation of the expense of what I had first done he ought to have charged; but its being done at a distance of twenty miles from where he lives, and in the most busy time of the year for masons work, I suppose must account for it in the first instance; and having once made that price, he would not now work under: but, I believe, for a considerable building, and with sufficient notice, and being allowed 6*d.* per mile in lieu of wages and travelling expenses for himself and

and an assistant, out and home, he would go to any part of the kingdom, on being paid 8*d.* per yard for the work. It has been the general received opinion here, that plaster made with sea sand, unless well washed in fresh water, would always be damp; but, on the contrary, I find from what has been done in my dining-parlour and passage, it has been always quite dry, although the whole of the sand with which it has been done has been thrown up by the sea, and must have been always at spring tides covered with sea water: indeed it sometimes happens, that, for months together, there is none to be collected on our shores at this place, that Everett thinks fine enough for the purpose; and as I am now and then applied to for getting it, I have lately, when my horses were at leisure, got a small quantity collected and hauled in for my own use, or, in case of its being wanted, I charge 2*d.* per cwt. for it, where it is deposited. As I design at some future time to make some alteration in the passage done with the stucco in April 1806, I had four pieces taken off, which I tied up separately, each in a piece of brown paper, and had them packed in a box, with a layer of sand between each piece, and at the bottom and top of the box, and directed it for you, and sent it with some goods I shipped on Saturday last to my friend Netlam Giles, Esq. No. 2, New Inn, St. Clement's. I have requested of him, that he will have the goodnes on its arrival to forward it to you. The vessel it goes by is the sloop Mary Ann, John Anning, master, bound to Dounes Wharf, Hermitage, Wapping. It is possible, that the pieces of stucco sent may imbibe some of the saline particles of the sand it was packed up in; but this did not occur to me at the time or they should have been packed in saw-dust; as they were perfectly dry when packed, so much so as, when struck upon with the knuckle, to give a sound similar to what an earthen vessel would do if dry and not cracked. Should there be any further information requisite, on your letting me know, it shall immediately be sent you. It had almost escaped me to say, that the small quantity of six yards, done last October with stone lime for trial, was done from your intimating

Sea sand not liable to get damp.

Hardness of the stucco.

Stone lime appears to answer the purpose.

intimating to me, when I had the pleasure of seeing you in Dorsetshire, that stone lime was likely to answer; but it would I think look better if white washed; the difference in point of expense is materially in favour of the stone lime. The cost of my waggon-load of it at the kiln, about a mile hence, would be only 10*s.*, whereas about the same quantity of chalk lime at the kiln, full eight miles from hence, would cost 1*l.* 4*s.*, and I cannot get any chalk-lime nearer. I have only now to add, that I am, very respectfully,

Sir,

Your obedient humble servant,

BRIDPORT HARBOUR, April 22d 1811. H. B. WAY.

Certificate.

Account of stucco work done.

I hereby certify, that Mr. H. B. Way, merchant, of Bridport Harbour, in the county of Dorset, in the month of March 1805, gave me the necessary directions for making a strong cheap stucco or plaster, which was composed of one part chalk lime, and three equal parts of fine sand, collected on the seashore, near Bridport Harbour, the whole of which was mixed up to a proper consistence with a strong lime water; and I have since that time done the annexed work with the said composition.

For Mr. H. B. Way, at Bridport Harbour.

	Yds.	Yds. flat Msr.
1805. March.—On the outside of a warehouse wall, part rough stone and part brick	25	
1806. April.—On the inside walls of a passage in his dwelling-house, on rough stone.	10	
Oct. & Nov.—On the inside rough stone walls of two cellars.	224	
One coat on the ceilings of the said cellars	228	
N. B. The first coat on the ceilings was common hair mortar		
1807. April & May.—The whole of the outside of hisdwelling-house, rough stone walls	335	

	Yds.	Yds. flat Msre.
August.—On one side wall of the dining-room in brick; this stucco was rubbed down quite smooth, and has since been painted with oil colours ……	13	
1810. Oct. 10.—On a rough stone wall of a warehouse directly fronting the sea, and not two hundred yards from it, with common stone lime, such as is used for manure in this quarter, by way of trial …………………………………	6	
	——	821

1811. April.—At Mr. H. B. Way's request, I have this day carefully examined the whole of the above work, and I find it sound and good, and by his directions, four pieces of the stucco were taken off from the passage wall, (which was laid on April 1806), and packed in the same sort of sand as is used in the composition, and sent by him directed for the Secretary of the Society of Arts, Manufactures, and Commerce, London.………………

For Peter Daniel, Esq. of Yeovil, Somersetshire.

1808. May & June.—On the outside brick-walls of his dwelling-house there. ………………	430	
1810. May & June.—On the outside brick-walls of other dwelling-houses there………………	480	
	——	910

For W. Hellyer, Esq. of East Coker, near Yeovil.

1809. June.—On the outside brick and rough stone walls of his dwelling-house, at that place. ‥		1040

For the Rev. Joseph Fawcett, of Yeovil.

1810. June.—On the outside rough stone walls of his dwelling-house there ………………		212
N. B. Mr. Fawcett's house being built the year before, with a view to being stuccoed, the walls were left rough.		—— Yds. 2983

I hereby certify, that the whole of the foregoing two thousand nine hundred and eighty-three square yards of stucco, were done with the before-mentioned composition, by me and my men under my directions; and I verily believe it is

the cheapest stucco known, and that it will prove very durable, both without doors and within, and that it has given entire satisfaction to the gentlemen who have tried it; and I am now engaged, if I can, the ensuing summer, to stucco the outside of one house at Bridport, and another at Yeovil, also the inside of a cottage for labourers that I have just built for Mr. H. B. Way, at Bridport harbour.

THOMAS EVERETT.
Stone Mason, Bricklayer, and Plasterer.

Shipton George, near Bridport,
Dorset, April 22, 1811.
Witness, JAMES BUDDEN.

VII.

Manufacture of Cloth and Cordage from Nettles, by Mr. EDWARD SMITH*.

Cordage and cloth from nettles.

IN page 109 of the 28th volume of the Society's transactions† will be seen a communication from Mr. E. Smith, of Brentwood, on manufacturing a variety of articles from the fibres of the common nettle, for which he has received their silver medal. He has since, with great attention and laudable industry, extended his experiments on this subject, and, during the last session, produced to the society specimens of cloth and cordage made from the nettle, which appear to possess great strength and durability. The society have, therefore, this session, voted to him their silver Isis medal. The following communication was received from him, and specimens of the cordage and cloth, made by him from nettles, are preserved in the Society's repository.

ESTEEMED FRIEND,

Introduction of a new substance of productive labour.

I received thy kind favour of the 23d instant, by the contents of which I am much obliged; and being impressed by the consideration of the vast importance the introduction of

* Trans. of Soc. of Arts, vol. XXIX, p. 81.
† See Journal, vol. XXIX, p. 161.

a new

a new substance of productive labour would be of to the community of this manufacturing country, particularly as affording a new source of industry to the increased numerous poor of both sexes, in truth, so operated on my mind, as to create a great unwillingness to suffer any exertions consonant with my limited powers, from total disadvantages, to lie dormant. I am, therefore, very desirous by unremitted endeavours to be instrumental in disseminating the knowledge of, and the means of bringing into use, so valuable a spontaneous production as the common nettle substance, under the sanction and through the medium of the enlightened Society of Arts &c. These considerations, aided by the hope of obtaining their farther approbation, have stimulated me to persevere in my attempts to contribute all in my power towards the advancement of so desirable and beneficial an object; in the expectation, that when all the different fabrics, which that substance is capable of being converted into, are produced, it may have a greater tendency towards encouraging others to embark in a manufactory thereof, than volumes written on the subject. With these sentiments I am induced to trouble thee farther, in requesting thou will be so kind to favour me by laying before the Society the different specimens of manufactory from the nettle substance, which I have at present in readiness, and which will be sent to thee by the Brentwood coach, which inns at the Blue Boar, Aldgate, and I expect will be delivered soon after the receipt of this. The cordage Nos. 1 & 2 is affirmed by the cord-spinner to be of equal strength to that made from hemp. The cloth No. 1 is rough from the loom; No. 2, the same fabric half bleached; and No. 3, which I ordered to be *huckaback*, also is only half bleached for want of sufficient time for the process. The quality of the cloth hath suffered throughout, by the negligence or willfulness of the manufacturer, and is principally owing to the raw material having been only passed through such heckles as are used for the coarse part of the hemp manufactory;—other necessary operations were omitted, in consequence of my instructions not being attended to by the person into whose care it was entrusted. He resides in the country, at a great distance, and his capability and integrity proved greatly inferior to the opinions I had entertained

Fabrics from the nettle.

Cordage.

Cloth.

This injured by the manufacturer.

him; and it now appears his practice is confined to the coarser part of the hemp manufactory. It was my intention to have produced with the above a pair of stockings, manufactured on the principle of cotton, and also a specimen of fine cloth on the same principle, with a view to show the great extent of contrast; but, on application to a cotton spinner, I found the quantity of material I had in a state of preparation suitable was not sufficient for the operations of carding; in consequence I am obliged to postpone my designs till I am enabled to prepare a sufficiency. Greatly desirous of contributing to the accomplishment of the object in view, and sensibly how much the sanction and approbation of the Society would tend to promote it, I hope they will consider my continued exertions worthy their farther attention. Anticipating their approbation, I remain,

Very respectfully,

Thy assured Friend,

EDWARD SMITH.

Brentwood, the 26th of 3d Month, 1811.

VIII.

Account of Herrings cured in the Dutch mode on board British Vessels; *by* FRANCIS FORTUNE, *Esq.**

Fishing for herrings.

IN the deep sea (which is the principal fishery for herrings) the nets are cast from the busses by sunset, and they drive by them alone expecting the shoals, the approach of which is generally indicated by small quantities of fish; and their arrival by immense flights of sea fowl. The best fishing is with the wind off shore, for, when it blows in a contrary direction, the shoals are broken and dispersed, and the fishery is seldom successful while it continues in that point.

Management of them when caught.

Immediately after the nets are hauled in, (which is often performed with considerable difficulty, by means of a wind-

* Abstracted from the Trans. of the Soc. of Arts, vol. xxix, p. 157. The gold medal, being the premium offered, class 165, for curing British white herrings in the Dutch method, was adjudged to the author.

lass

lass when they are full) the crew begin to gyp the fish, that is, to cut out the gill, which is followed by the float or swim, and divide the large jugular or spiral vein with a knife at the same time, endeavouring to waste as little of the blood as possible;—at this work the men are so expert, that some will gyp fifty in a minute.

Immediately after they are gypped, they are put into barrels, commencing with a layer of salt at the bottom, then a tier of fish, each side by side, back downwards, the tail of one touching the head of the other, next a layer of salt, and so alternately until the barrel is filled:—they are thus left, and the blood which issues from the fish, by dissolving the salt, forms a pickle infinitely superior to any other that can be made. The herrings thus drained of their blood occupy less space, and the whole consequently sinks about one third down the barrel, but this sinking is at an end in about three or four days. Salting.

When these operations are being performed, the sea is often running mountains high; and it is not therefore to be supposed, that the barrels are so well coopered as not sometimes to allow the pickle to leak out; and in order to preserve the fish from being spoiled, which would otherwise happen in such cases, some of the gills and entrails are always put by in barrels with salt, in the same manner as the herrings, and yield a pickle of the same quality; with this pickle those barrels which have leaked are replenished, and the fish sustains no injury. Every operation is performed in the shade, into which the fish are immediately conveyed on their being hauled on board. Each day's fishing is kept separate with the greatest care. The salt used is mixed, and of three different sorts, viz. English, St. Ubes, and Alicant, and each barrel marked with the day of the month on it on which it was filled. Precautions against loss of pickle from leaking. Fish kept in the shade, each day's separate. Salt used.

The advantages of gypping the herrings are, that the blood, which issues in consequence of the operation from the fish, yields a natural pickle, and improves the flavour; whereas, if left in the fish, it becomes coagulated at the back-bone, and forms the first cause of decay. The mixture of blood and salt operated upon by the extreme heat of the weather during the summer fisheries produces a fermenta- Advantages of gypping.

tion

tion which nearly parboils the herrings, and removes the coarse and raw flavour so often complained of. The gypping is likewise often performed on shore, observing the same precautions; the only difference is, that they are seldom in that case of so good a colour. Gypped herrings are never of so fine a quality as when kept in their own original pickle; their value consists in their softness and flavour; it is this mode of curing herrings that used to be the pride of the Dutch, and this is the kind which supplied their home consumption, and were so much esteemed by all classes of people in Holland.

Difference in their value.

In order, as far as it is possible, to give a proof of the correctness of the above assertion, I shall state a fact for the information of the Society. During the last year I employed a number of Dutch fishermen, prisoners, and others, with Englishmen, in gypping and curing herrings; and at one time my agent at Yarmouth was offered £4 per barrel, for all the herrings he had cured there, by a Dutch captain, in order to their being taken to Holland, while ungypped herrings were worth only 36s. per barrel. The herrings now under the consideration of your Society are part of the quantity for which this offer was made.

Should the Society, after due consideration, think proper to adjudge me their gold medal, it will afford me much satisfaction, and convince me, that my exertions have, in some degree, been beneficial to the community.

I am, Sir,

Your most obedient servant,

FRANCIS FORTUNE.

No. 9, Lower Thames Street,
Feb. the 26th, 1811.

IX.

IX.

Method of Curing Herrings: by Mr. SLEAVIN*.

WHEN the herrings are taken and alive, break their gills with your finger and thumb completely from the back-bone, which will in course cause the fish to bleed: then throw them into the liquor prepared as follows: viz. to three quarts of salt water, put five pounds of common salt, and two pounds of bay salt, and when dissolved, let the whole be boiled. One peck of common salt, and half a peck of bay salt, put between the different layers of herrings, will be sufficient for one barrel. Let the herrings remain in this liquor for three weeks, they must then be taken out and gypped, and a fresh liquor made with one gallon of salt water, the gypping of the fish, one peck of common salt, and a quarter of a peck of bay salt, and when dissolved, some of the spare fish mustbe put in to it to make the liquor rich, and the whole be boiled for an hour, ut so slow as that it may not burn; then let it cool and strain it off. The fish must be repacked in clean barre the last mentioned liquor put to them, and be careful that the fish be covered and kept close.

Another mode of curing herrings.

P. SLEAVIN.

No. 7, *Little Brook Street, Hampstead Road,*
April the 6*th*, 1811.

X.

Letter on the Structure of the Water L[illegible] in answer to a Correspondent. By Mrs. AGNES IBBETSON.

SIR,

THE gentleman, who did me the honour to notice my letter on water plants, in your last number, p. 22, is per-

Structure of the water lily.

* Abstracted from the Trans. of the Soc. of Arts, vol. XXIX, p. 162. Mr. Sleavin cured only eight barrels, of thirty-two gallons each, of herrings caught off the Isle of Man. Nothing is said of their quality, except Mr. Sleavin's assertion, that he has no doubt they are equal to the Dutch, or better. The silver medal of the Society was [illegible]ted to him.

fectly

fectly right: the mistake of my amenuansis, who inserted "washed off" instead of "rubbed off," has caused an apparent confusion in the description. No water can enter the air vessels, except when the adjoining parts are much torn.

Motion of the hairs.

The motion of the hair in the air cylinder is caused by a pin, which, entering the widest end of the hair, runs through the side of the air vessel into the next water vessel. The water rising contracts the spiral, pushes the pin, and the hair, which remained before parallel to the side of the vessel, now starts up horizontally; and, as the whole circle of hairs rises, each in the same manner and at the same time, meeting with their points, if any insect has placed itself in the way, it will be crushed or run through (as I have often seen it) by the sudden motion of the hairs.

The plant the food of many small slugs,

and perhaps of the cicaria lemnæ.

No insect certainly can get into the air cylinders, but by the dilapidations of some of the adjoining vessels; but this must often happen, as these plants are the food of many of the diminutive slug kind; and I doubt whether the cicaria lemnæ (which is the only species I have found on the hairs) do not also feed on it.

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

XI.

On the Irritability of the Sowthistle, and other Plants, with farther observations on the Irritability of Vegetables: by D. J. CARRADORI*.

Irritability of plants.

Shown in the sowthistle,

THE lettuce is not the only plant that possesses a striking degree of irritability during the period of flowering, the prickly sowthistle *(sonchus asper)* has this faculty at the same season. In fact it transmits and gives out a milky fluid, like the lettuce, when it is irritated, or punc-

* Abridged from the Journ. de Phys. vol. LXVII, p. 405.

+ Experiments on the Irritability of the Lettuce, Mem. of the Italian Society of Sciences, vol. XII.

tured

tured, at that time; though not so quickly as the lettuce, or with the same facility and force. It requires a stronger irritation, or a more powerful and complex stimulus, to excite the flow of the milky liquid in this plant; and does not obey the slightest touch like the lettuce, which, as soon as it is touched, however gently, throws out a portion of its proper or milky juice.

This exudation is never performed with the same force as in the lettuce, from which it is sometimes spirted out into the air to some distance; but simply flows out, however powerful the irritation. Neither is it obtainable from the leaves that embrace the stalk, as in the lettuce, but from the calices alone, and chiefly from the circumferences of the little leaves that compose these. and more strikingly in the lettuce.

The sowthistle, like the lettuce, does not lose this faculty when immersed in water; and the plant, if pulled out of the ground, or a single branch of it, will retain it some time. Not destroyed by immersion in water.

I have not had time to extend my observations to the other species of the lettuce and sowthistle, to find whether, either while flowering or at any other time, they gave signs of a sensible degree of irritability in any part by a similar exudation, though this is probable. I have found it in the bark of the fruit when green, or in the pericarps of these plants. Found in the pericarp.

I could not obtain the customary exudation from the leaves, the stalks, the parts that support the organs of fructification, or any other part, in whatever way I irritated them, except from the green capsules containing the seeds: and the irritation was always produced by a needle, or rubbing; never by any method capable of tearing or injuring the surface of the capsules*. Other parts void of it. How excited.

* There are motions in plants not owing to irritability, but the simple effects of the elasticity of certain parts, as in the great mullein *(verbascum sinuatum)*. If a shock, or commotion, be given to the stalk of this plant, the flowers will fall off; not immediately, nor all at once, but a little while after, and successively. This is owing to the elasticity of the calices, which are kept in a state of forced distension to hold the monopetalous flower, which is not attached to them; and as the shock causes them to contract, by calling into action their natural elasticity, this contraction gradually expels the flower. The mullein made to shed its flowers.

I find

Effect of saline impregnations on the irritability:

I put some small lettuce plants, while in flower, with their roots, to vegetate in water, to which was added in some of the vessels a small portion of muriate of soda, in others of nitrate of potash, and kept them thus in the open air some days. If any portion of these salts were absorbed from the water, they did not appear to increase the irritability of the plants, for the exudation diminished in quantity.

of acids:

I mixed acids also in water, in such proportion as to be barely sensible to the taste, and particularly nitric and oximuriatic acid in various proportions, and then placed several small plants of lettuce in full flower in the vessels, with similar effect. If a larger quantity of acid were added to the water, it appeared to injure the plants, and their irritability likewise decreased more quickly.

and of oximuriatic and other gasses.

I applied this kind of stimulus to the surface of plants, to see if it would act externally. On immersing a branch of lettuce in a jar filled with oximuriatic acid gas, and taking it out in a few seconds, it exhibited the usual exudation when stimulated: but when it was kept longer in the gas, it was evidently injured, and its irritability greatly decreased. Nitrous and sulphureous vapours were still more injurious.

It appears then that these stimuli, which are so much vaunted as inceasing the irritability of animals, are not appropriate to the irritability of vegetables*.

* Those facts, that are admitted as proofs of the stimulant action of certain substances or principles in the vegetable economy, do not appear to me decisive.

Oxigen does not act as a stimulus to vegetables.

It is generally supposed, that oxigen is a powerful stimulus to vegetables, because it has been observed to accelerate the germination of seeds; this effect being ascribed to its stimulating their vascular system, and rendering their circulation more active. But as it appears from the observations of Mr. de Saussure the younger, that the oxigen entering into germination is neither absorbed nor assimilated in this process, but employed in forming carbonic acid, I conceive it does not act as a stimulus, but merely serves to carry off from the germinating seed the carbon; an element which, as is shown in some of my observations respecting the action of light on germinating seeds, inserted in the *Opuscoli scelti* of Milan, seems injurious to the developement of the embryo; and of which nature seems at that time disposed to free herself, as noxious or superfluous. This appears to be the reason why oxigen accelerates germination.

I immersed

I immersed lettuce plants in some stagnant water, from which fetid exhalations rose, and kept them in it twenty-four hours. Having taken them out, and repeatedly examined the state of their irritability by stimuli, I found they had lost it entirely. The vessels containing the proper juice of the plants were so deprived of irritability, that they did not emit their fluid, even though wounds were made in the plants for the purpose. It seems therefore, that putrid exhalations, or putrid matter combined with water, deprive these plants, as well as animals, of their irritablity. Lettuce immersed in stagnant water:

Having taken a lettuce plant, when Reaumur's thermometer was at 25° in the shade [88·25° F.], I immersed it in water at 50° [144·5° F.]; a degree of heat I had found not to injure the organic texture of vegetables. In this hot fluid there was a spontaneous exudation from the plant; and at the slightest touch it gave out its juice more freely than in air at the same temperature. I then immersed it for a moment in water at 4° [41° F.]; and, after waiting for a few seconds, that it might have become sensible of the effect, I irritated it afresh; when I found it required a much stronger irritation. exposed to a high and low temperature.

The irritability of vegetables appears to be increased by heat, and diminished by cold. Vegetables in fact slacken in the exercise of their functions, if they do not suspend it entirely, during the cold weather; and the spring, which brings with it warmth, restores to the vegetable economy its accustomed energy. By this it appears their sleeping irritability is awakened, and their life revived; so that the state, in which vegetables pass the winter, may be compared with the torpor, or lethargy, that many animals undergo during that season. Cold benumbs animals, because as is well known, it deadens their irritability; and this it does by its direct action on the muscular fibre, which is the seat of irritability, independent of sensation and circulation, as Spallanzani has shown. Irritability of vegetables increased by heat and diminished by cold.

Into a deep well, where the thermometer was at 12° [59° F.], I put a plant of lettuce in full flower; taking it from a kitchen garden, where the thermometer stood at 26° [90·5° F.] in the shade: and kept it there some hours, with its root only in A moderate temperature does not affect it.

in the water. Having taken it out, I found by repeated trials, that the exudation followed irritation, as it appeared to me, nearly as before; so that I could not find any perceptible difference in its irritability after it had been exposed to this cool temperature.

Thus the irritability of vegetables does not appear to suffer by a sudden transition from a high to a moderate temperature, or to be diminished in proportion to it: though the preceding experiment shows, that, when their irritability has been heightened by a very hot atmosphere, and they are placed for an instant in a cold one, it is perceptibly diminished.

Light has no effect on the irritability.

Light is well known to act as a stimulus on plants: but I did not find greater marks of irritability in the lettuce or sowthistle when surrounded by sunshine, than when in the shade.

I tried the effects of the solar light concentrated by a lens on these two plants; but it did not produce any irritation, so as to cause the exudation of the usual fluid, though it scorched them, when sufficiently intense.

Life and irritability extinguished together.

I pulled up some whole plants of lettuce and sowthistle, and also stripped off some branches, and left them to wither on a table in my room in summer. About ten hours after I irritated them where the effect would be most visible, and obtained some slight marks of irritation. I then placed the stalks of one or two of these plants in water; and after some time I found they recovered from their apparent death, and began to vegetate afresh. A little time after I attempted to irritate some others, that were still more withered; but they exhibited no exudation. I then put them in water like the former, but they never recovered. Thus in plants life and irritability appear to become extinct together.

Irritability strongest in the morning.

I tried to irritate plants of lettuce and sowthistle, growing in the same ground, at various hours of the day and night; and I found their exudation most energetic in the morning, when the sun had risen, and their flowers were fully expanded.

This irritability probably common to all plants,

The property, that lettuce, sowthistle, and spurge have, of giving out a milky fluid, or their peculiar juice, when any of their more succulent parts are irritated, appears to me, to render the existence of irritability in plants unquestionable.

It

It is true, that we perceive this irritability only at a certain age, and not in all plants that have a peculiar juice. But are we to presume, that, if this property do not manifest itself at every age, and in every plant, but only when it is extremely exalted, and in those plants that are perhaps most endued with it, other plants are destitute of it? On the contrary we may reasonably infer, that those vessels, which exhibit a great deal of irritability at a certain period, and in certain plants, possess at other times, and in other plants, a sufficient quantity for the circulation of the fluids, though no excess of it to be rendered sensible.

But if it be reasonable to suppose, that the vessels containing the peculiar juice are endued with this irritability, and that it is by this property the juice is compelled to circulate in them; who will venture to assert, that the vessels of other systems are destitute of it, and that the circulation of their respective fluids arises from a different cause, or is occasioned by some other power? and to all their vessels.

XII.

Chemical Examination of some Vegetable Substances; by Mr. VAUQUELIN*.

SECT, I. *Chemical Examination of a vegetable excrescence from Madagascar, sent to the Isle of France by Mr. Chapellier, and thence to Europe by Mr. Jannet.*

THIS substance is as white as a cake of starch; it is perforated in all directions by an immense quantity of holes formed by little insects; it has neither smell nor taste; it diffuses in burning the smell of burned bread, inclining a little to that of touchwood. Vegetable excrescence from Madagascar described.

1. Treated with a very large quantity of nitric acid, it furnished a little oxalic acid, but no muric; consequently it contains no gum.

2. Water has no action on it: but if it remains a long time

* Ann. de Chim. vol. lxxii, p. 297.

time in this liquid, at a temperature sufficiently high, a portion of the animal matter, which appears to be contained in it, undergoes putrefaction, and imparts to the water a fetid smell, analogous to that of cauliflowers; which appears to indicate the presence of sulphur.

The portion that remains still enjoys all its properties.

3. Acetous acid, boiled with this substance, takes from it some matter, which appears to be of an animal nature; for it is precipitated by galls, but not by alkalis. What is not dissolved by the vinegar possesses the same properties as before, or at least nearly so.

3. Ten grammes [154·45 grs] of this matter, subjected to distillation, yielded an empyreumatic oil, mixed with an acid liquor, which diffused an ammoniacal smell, when potash was mixed with it.

The coal, when burned, left 1 dec. [1·54 gr.] of yellowish ashes, containing a little phosphate of lime, some carbonate of lime, and a trace of oxide of iron.

This matter having the appearance of starch, or at least seeming to contain some, a principal object of all the experiments made with it was to discover this; but not the least trace of it could be detected.

Result. From this examination it seems to result, that the substance is a mixture of unorganized woody matter, and of vegeto-animal matter, which, having been superabundant in the vegetable, were expelled to its exterior, and there formed an excrescence.

SECT. II. *Analysis of a gum-resin, sent in the year* 13 *from Madagascar to the Isle of France, by Mr. Chapellier, and thence to the Museum of Natural History by Mr. Victor Jannet, in November*, 1808.

Gum-resin from Madagascar. This gum-resin is of a greenish brown colour. It burns swelling up, and emitting a thick smoke, with a smell not very pleasant; and leaves ashes containing carbonate of lime.

Analysed. Alcohol, assisted by a gentle heat, dissolves a great part of it; leaving a residuum greasy to the feel, which alcohol attacks only when boiling, and the greater part of which separates

separates immediately on cooling. The matter that thus falls down in cooling exhibits all the properties of lac. Its weight is six hundredths of the resin.

The portion of the resin, one tenth, on which the alcohol had no action, was treated with caustic potash dissolved in water. This had not much more action on it than alcohol; leaving it in the form of a brown powder, soft to the touch, and still weighing near one tenth.

This substance, insoluble both in alcohol and potash, was distilled with a gentle heat. At first it gave out a little water; and then vapours arose, which condensed into an oil, and a liquid of a taste somewhat aromatic, without being disagreeable, having a great resemblance to the product of gums.

None of the products of this distillation, mixed with quicklime or with potash, yielded the least trace of ammonia. The coal in the retort was easy of incineration, and left a decigramme of yellowish ashes, containing some lime, and a little oxide of iron.

The alcoholic solution of the resin had a brown colour and a peculiar taste. Being evaporated to dryness in a retort, the alcohol that came over contained nothing aromatic.

The resin was boiled in water, to which it communicated a slight taste. Thus purified it had a yellowish brown colour. It retains water pretty strongly, for it is difficult to dry, and remains soft a pretty long time.

Component parts.

Thus it appears, that the substance which we have called a gum-resin contains, in 10 grammes,

1, Lac	0·6
2, Residuum, containing a little more lac and vegetable matter	1·0
3, Remains for the weight of the resin	8·4
	10.

One plant may yield different resins, and different plants the same resin.

This is I believe the first time, that lac has been found mixed with other resins; and this fact confirms us in the opinion, that the same vegetable may form several kinds of resins, as well as different trees produce the same resin.

SECT. III. *Analysis of the root of camel's hay, andropogon schœnanthus, L., sent from the Isle of France by Mr. Jannet, in* 1808.

Root of sweet rush.

This root has a yellowish colour, and in smell resembles Virginian snakeroot.

Treated with alcohol,

Twenty grammes [308·91 grs] were infused in alcohol, which was renewed, till it no longer acquired any colour.

The filtered alcoholic solutions had a fine golden hue. Subjected to distillation, alcohol came over, the first portions of which had no foreign smell; but as the liquor in the retort became less spirituous, and required more heat to keep up the ebullition, the weaker spirit that came over had a perceptible smell, a little resembling that of the root.

The matter remaining in the retort became turbid, and was decanted boiling hot into a capsule. On cooling it let fall a brown oil.

The supernatant liquid had a yellow colour; and a very little taste, slightly saline, and a little aromatic. The oily sediment was thick, smooth to the touch, had an acrid, burning taste, like an essential oil, and in smell greatly resembled myrrh.

with water,

The 20 gr. of the root, after being exhausted by alcohol, were boiled in water. The decoction, after being concentrated, had a yellow colour, and very little taste; it did not precipitate sulphate of iron or gelatine; it was not rendered turbid by alcohol or infusion of galls; it reddened infusion of litmus pretty strongly, but, as the liquor was in small quantity, the nature of the acid could not be ascertained: thus the alcohol had left the water scarcely any thing to dissolve.

and dilute nitric acld acid.

After the root had been boiled in water, it was infused in diluted nitric acid. This infusion gave with ammonia a very slight precipitate, which resembled oxalate of lime; but there was too little of it, to be certain of its nature.

Incinerated.

20 gr. [308·91 grs] being incinerated left a red residuum weighing 8 dec. [12·36 grs]. This residuum dissolved in muriatic acid with a very slight effervescence. The solution had a fine yellow colour, and gave with ammonia a bulky precipitate of a deep brown colour. Treated with caustic pot-

ash,

ash, this precipitate afforded a little alumine; but the alkaline liquor did not give the least trace of phosphoric acid.

The ammoniacal liquor, from which the oxide of iron had been separated, yielded a little lime to oxalic acid. The residuum left by the caustic potash was oxide of iron.

Thus this root contains,

1, A resinous matter of a deep brown red, with an acrid taste, and a smell exactly similar to that of myrrh. In fact we believe it is nothing but resin of myrrh. Substances contained in it.

2, A colouring matter soluble in water.

3, A free acid.

4, A calcareous salt, the species of which we could not ascertain.

5, Oxide of iron in pretty large quantity, the state of combination of which in the plant we do not know.

6, A large quantity of woody matter.

The most interesting result of this analysis is the presence in the andropogon schœnanthus of a resinous matter, altogether similar to the resin of common myrrh; it differs only by being a little less solid, but if it were mixed, as in myrrh, with a certain quantity of gummy matter, I have no doubt it would resemble it perfectly. Hence we may infer, that myrrh is formed in several vegetables; for, though we are unacquainted here with the tree from which the myrrh of the shops is derived, it is probably not the andropogon schœnanthus. Myrrh may be formed in several plants.

Sect. IV. *Analysis of the aromatic leaves of the raventsara, agathophyllum ravensara L, sent by* Mr. Thouin.

I digested 15 gr. [231·68 grs] of these leaves in alcohol at 36° [0·837], to which they gave a fine green colour. I repeated the digestion, till the alcohol acquired no colour assisted by heat. Leaves of raventsara treated with alcohol.

The solutions, when mixed, were of a fine green. On cooling a small quantity of flocculent matter separated, which I found to be wax.

The alcohol, freed from this matter, was distilled in a retort. The spirit that came over had a very pleasant smell and taste.

The remainder was rendered turbid by a little green vegetable resin. When this was separated by filtration, the liquor was of a fine brown yellow. On standing a small

quantity of brown matter was deposited ; after which a few drops of oil collected on the surface having the taste and smell of oil of cloves.

Yielded an oil.

The liquor, evaporated spontaneously in the open air, yielded a pretty considerable quantity more of this brown oil, and a clear liquor, as thick as a sirup, which had the taste of oil of cloves mixed with bitterness.

The leaves boiled in water.

I boiled in water the leaves exhausted by alcohol, but they only imparted to it a slight yellow colour, and the property of faintly reddening infusion of litmus, and being copiously precipitated by alcohol. This decoction was not affected by infusion of galls, solution of sulphate of iron, or gelatine.

and incinerated.

After the leaves had been drained, I incinerated them, and from the 15 gr. [231·68 grs] employed obtained 7 dec. [10·81 grs] of carbonate of lime, mixed with a little phosphate of the same earth.

As it is to be presumed, that this lime was combined with oxalic acid in the leaves, I digested 8 gr. [12·36 grs] in nitric acid diluted with a great deal of water ; but the acid liquor yielded a very little precipitate when saturated with ammonia.

The oil similar to that of cloves.

The oil we obtained from the raventsara exhibited absolutely all the properties of the essential oil of cloves ; its colour, smell taste, and specific gravity, which is a little greater than that of water. It differed only by being a little more consistent, which was probably owing to the leaves being old, so that the oil had been thickened, and in some sort resinified, by time.

Different plants may form the same oil.

From this analysis we may infer, that vegetables of different species are capable of forming an essential oil of the same nature.

The leaves a substitute for cloves.

These leaves might be employed for domestic purposes instead of cloves, using them only in larger quantity.

XIII.

Of the Efficacy of Plumbago against Tetters; by Dr. WIENHOLD*.

Plumbago employed as a medicine.

PLUMBAGO is a natural compound of nine parts carbon with about one of iron†, forming the carburet of iron of the

* Ann. de Chim. vol. LXXVI, p. 198

† Carbon 96, iron 4. C chemists.

chemists. No one seems to have thought of introducing it into the materia medica, unless in the polar regions, where the people not only rub themselves with it daily, but employ it against cutaneous eruptions. This fact, added to its known property of exciting animal electricity, and conducting it like metals, induced Dr. Wienhold to make trial of it: and in the General Medical Annals of Altenburg, for May, 1809, he published his observations and remarks on it; from which he affirms, that he can recommend it by experience against all tetterous eruptions; as, whether simple or complicated, they yield to its internal and external application, provided it be joined with medicines appropriate to their different complications; as iron, muriate of lime, and dulcamara, in scrofulous; aconite and guaiacum in arthritic; mercury in siphylitic; and sulphur in psoric tetters.

in herpetic diseases.

In the latter, which neither sulphur alone nor black lead alone would cure, he has always been speedily successful, on giving the patient daily a drachm of graphitic ethiops, made by triturating together equal parts of sulphur and plumbago.

Graphitic ethiops.

We shall not here enter into all the modes of administering this remedy, which the author has varied according to the cases; the formulæ he has given for their preparation; and his remarks on their mode of acting; which may be seen in No. 85 of the *Bibliothèque médicale*, we shall only add, that, for want of English black lead, being obliged to use that of Passau, he found, that it was less efficacious, required to be given in a larger dose, and, not being reducible to so fine a powder, did not sit so easy on the stomach. It is indeed well known, that the plumbago of Passau, though it does not contain pyrites like that of Spain, is much more loaded with foreign matter. To those who may be inclined to try this remedy however, we believe we may point out as preferable, on acconnt of its purity and the fineness of its grain, that which is found in the valley of Lucerne, or of Pellis, in the circle of Pignerol, in the department of the Po, where it forms a vein two feet thick by three broad, according to the description given by Mr. Bonvoisin in the Mem. of the Ac. of Turin, 1805, p. 182.

Plumbago differs in quality.

XIV.

XIV.

METEOROLOGICAL JOURNAL.

1812.	Wind	Pressure. Max.	Min.	Med.	Temperature. Max.	Min.	Med.	Evap	Rain	
4th Mo.										
April 4	E	30·10	29·60	29·850	53	36	45·5	—		☽
5	S	30·18	30·10	30·140	56	35	45·5	—	4	
6	S	30·10	29·93	30·015	53	42	47·5	·25		
7	E	30·02	29·88	29·950	50	37	43·5	—	·10	
8	N E	30·15	30·02	30·085	49	25	37·0	—		
9	N E	30·15	30·01	30·080	43	33	38·0	—		
10	N E	30·01	29·98	29·995	47	36	41·5	—		
11	W	29·98	29·89	29·935	44	37	40·5	·43		●
12	N E	30.05	29·89	29·970	52	32	42·0	—		
13	N E	30·05	30·05	30·050	51	33	42.0	—		
14	N E	30·05	29·91	29·980	51	32	41·5	—		
15	N E	29·91	29·73	29·820	53	30	41·5	—		
16	N	29·73	29·64	29·685	49	27	38·0	—	—	
17	N	29·83	29·64	29·735	48	28	38·0	—	—	
18	N	30·09	29·83	29·960	51	29	40·0	·87		☾
19	Var.	30·09	30·02	30·055	50	33	41·5	—		
20	N W	30·15	30·00	30·075	58	30	44·0	—	—	
21	N E	30·15	30·07	30·110	58	37	47·5	·28		
22	N	30·01	29·97	29·990	54	30	42·0	—		
23	—	——	——	——	52	32	42·0	—		
24	W	29·94	29·86	29·900	52	36	44·0	—		
25	S W	29·86	29·56	29·710	54	39	46·5	—	—	
26	Var.	29·65	29·55	29·600	49	34	41·5	·69	·47	○
27	N E	29·64	29·59	29·615	52	44	48·0	—	6	
28	N E	29·76	29·64	29·700	51	44	47.5	—	·14	
29	N E	29·80	29·76	29·780	52	43	47.5	·27	·39	
30	Var.	30·02	29·80	29·860	55	45	50·0	—	—	
5th Mo.										
May 1	E	30·02	29·92	29·970	59	43	51·0	—	—	
2	N E	29·92	29.73	29·825	54	42	48·0	—	—	
3	Var.	29·75	29·70	29·725	56	32	44·0	·55	4	
		30·18	29·55	29·902	59	25	43·57	3·34	1·24	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES.

NOTES.

Fourth Month, 4. Cloudy a. m. Clear evening. 5. Much dew: barometer unsteady: heavy clouds through the day: a shower about sunset. 6. Much dew: gray sky, and the air nearly calm. 7. Lightly cloudy: little wind. 8. Cloudy a. m.: a shower p. m. 9. Brisk wind: cloudy. 10. Hoar frost. 11. Cloudy. 16. Slight showers. 17. Little hail. 20. a few large drops. 23, 24. Occasional slight showers of hail, &c. 25, 26. Gentle showers of rain yet not warm. 27. Misty morning: much dew: swallows appear. 28, 29. Cloudy: windy.

Fifth Month. 1, 2. Cloudy: the cuckow heard. 3. About 1 p. m. a few drops of rain, attended with the smell of electricity in the air: the wind, which in the morning had been brisk at N. E., died away, the canopy of the sky rose: the evening was calm, and dew fell.

RESULTS.

Prevailing winds N. E.

Barometer: highest observation 30·18 inches; lowest 29·55 inches; Mean of the period 29·902 inches.

Thermometer: highest observation 59°; lowest 25°; Mean of the period 43·57°.

Evaporation 3·34 inches. Rain 1·24 inches.

PLAISTOW. L. HOWARD.

Fifth Month, 24, 1812.

XV.

Experiments on Camphoric Acid; by Mr. BUCHOLZ*.

DOERFURT imagined he had shown by experiment, that the camphoric acid, described by Bouillon-Lagrange, was Camphoric acid distinct

* Journ. de Phys. vol. LXX. p. 347. Translated from Gehlen's Journal by Mr. Vogel.

from the benzoic, was similar to benzoic acid. Bucholz has lately resumed the subject, and shown, that the camphoric is a peculiar acid. The following properties sufficiently distinguish them.

in crystallization, 1. The camphoric acid is crystallizable by slow refrigeration. The crystals, as Bouillon-Lagrange observed, greatly resemble plumose muriate of ammonia. The benzoic acid, on the contrary, under the same circumstances crystallizes in small needles, or in ribandlike laminæ.

taste, 2. The taste of camphoric acid is very sour, and leaves a bitterness behind; while that of the benzoic is sweet, saccharine, little acid, pungent, and excites coughing.

solubility in water, 3. Camphoric acid dissolves at 15° R, [65·75° F.] in 100 parts of water, and at a boiling heat in ten or eleven. Benzoic acid requires 24 parts of boiling water, and 200 at 15° [65·75° F.]

and in alcohol, 4. One part of alcohol at the common temperature dissolves 1·06 of camphoric acid; and 92 parts of boiling alcohol dissolve 146, or even more. Benzoic acid requires its own weight of boiling alcohol, and twice as much cold.

phenomena of sublimation, 5. Camphoric acid is capable of being sublimed as well as the benzoic, but the appearances are very different. In the first place it sublimes more difficultly: a great quantity is decomposed: an empyreumatic oil is produced with a smell of navew, an acid liquor, and a great deal of coal; and the sublimate has not a crystalline form. The benzoic acid sublimes in crystals, and yields no aqueous vapour, very little oil, and much less coal than the camphoric.

(Properties of the sublimed acid.) The camphoric acid when sublimed has a pungent and slightly acid taste. On account of the oil it dissolves more slowly in water. This solution reddens litmus paper.

and action on bases, 6. The camphoric acid comports itself very differently with respect to the salifiable bases.

particularly lime, The camphorate of lime exhibits a striking difference from the benzoate of lime.

A hundred parts of camphoric acid require for their perfect neutralization 56 parts of carbonate of lime; while the same quantity of benzoic acid requires $\frac{14}{40}$ parts.

The camphorate of lime crystallizes difficultly in rounded heaps

heaps; the benzoate, in shining stellar laminæ: The camphorate of lime has a slightly saline bitter taste, leaving a calcareous taste behind: the bonzoate is sweet, and a little earthy.

The camphorate of lime exposed to heat furnishes an aromatic oil, resembling that of rosemary in smell; no crystallized substance passes over; and the camphorate does not melt.

If the benzoate of lime be treated in the same manner, crystals of benzoic acid pass over into the receiver, with an empyreumatic oil having a smell of balsam of Peru; and the benzoate remaining in the retort becomes perfectly fluid.

The camphorate of lime dissolves in five parts of water at a common temperature; while the benzoate requires twenty parts.

XVI.

Inquiry concerning the Means of Knowing the Proportions of Acid and Potash, that enter into the Composition of Sulphate of Alumine, and of Sulphate, Nitrate, and Muriate of Potash: by Mr. CURAUDAU, *Prof. of Chemistry applicable to the Arts, and Member of various Literary Societies**.

Products of a large alum manufactory at variance with the admitted proportions of the salt.

IN undertaking the present inquiry I had no intention of verifying the experiments of those celebrated chemists, who have endeavoured to ascertain the quantities of acid and base, that enter into the composition of sulphate of potash; I was merely desirous of knowing why the annual results of the alum manufactory, that I have established at Vaugirard, were very far from agreeing either with the quantity of acid, or that of potash, which different analyses indicate as contained in sulphate of potash and in alum. For instance, when, instead of 31 parts of acid, the quantity designated as entering into the composition of 100 parts of alum, 43 or 44 are required; and, instead of ten parts and half of potash, fifteen and half are required for

* Journ. de Phys, vol. LXVII, p. 5. Read to the Imperial Institute, April the 4th, 1808.

100

100 parts of alum; such an increase in the quantity of materials could not fail to engage my attention, and lead me to seek the cause of so great a difference between the results of analysis and those of a manufactory on a large scale. At first I suspected, that the surplus of acid and potash I employed entered into the composition of the insoluble sulphate of alumine, which, I have remarked, is sometimes formed. Indeed I was long induced to entertain this opinion, rather than suppose, that the quantity of acid and of potash entering into the composition of alumine were more considerable, than had been fixed by different analyses, on the accuracy of which I had always depended.

An insoluble sulphate of alumine sometimes formed.

Desirable to prevent this.

However, admitting the hypothesis of the constant formation of an insoluble sulphate of alumine, I could not remain indifferent to the loss of this substance: on the contrary, it was an object with me to find the means of preventing the alum from passing to this state of insolubility. Accordingly, as soon as I was certain, that all the acid and potash employed entered into the composition of the alum I manufactured, I was convinced, that my former observations had been just.

Attempt to ascertain the proportions of potash and acid in alum.

But as I was not satisfied with being merely convinced of what was in favour of my observations, it remained for me to ascertain by direct experiments, and particularly such as could easily be repeated, how much acid and potash enter into the composition of alum. I wished also to learn, whether the quantities of acid and base in the sulphate of potash were such, as are generally admitted. Lastly, that my experiments might not be suspected of the least inaccuracy, it became necessary, that I should prepare some very pure sulphate of alumine; a circumstance that enabled me to obtain this sulphate very regularly crystallized, a state in which it had not yet been known, since its concentrated solution yields only lamellar, micaceous crystals, always of an irregular figure. I have had the honour of showing crystals of this sulphate of alumine to several members of the class, particularly to Mr. Haüy, who was very desirous of adding a specimen of this sulphate to his valuable collection.

Pure sulphate of alumine crystallized.

In

In a paper, which I shall have the honour of communicating to the class, I shall make known the physical properties of this saline substance, as well as the means and conditions requisite, to promote its crystallization.

This sulphate useful in analyses.

When I had at my disposal a certain quantity of this sulphate, it was easy for me to find with precision the proportions of potash and acid, that enter into all the salts with base of potash. I satisfied myself also, that this sulphate of alumine is a very powerful and certain test for ascertaining the quantity of potash contained in vegetables, either before or after incineration. On this subject I have undertaken several experiments, that will complete another inquiry, which I shall have the honour of submitting to the class.

To return to the analysis, or rather the synthesis, that constitutes the subject of the present paper: the following are the experiments I have made, to determine the respective quantities of acid and base, that enter into the composition of alum, and of the sulphate, nitrate, and muriate of potash.

Quantity of alum produced with a given weight of sulphate of potash.

Exp. 1. In 850 gr. [13129 grs*] of solution of sulphate of alumine at 34° [sp. gr. 1·307], the temperature being 10° [50° F.], I dissolved by the assistance of heat 100 gr. of sulphate of potash. After the liquid was cooled, I obtained from it 502 gr. of very pure alum. On evaporating the mother water I obtained 18 gr. of alum; and a second evaporation and crystallization produced 4 gr. more. The remaining liquor yielding no more crystals, I mixed it with 25 gr. of a solution of sulphate of alumine similar to that above, in order to find whether I had obtained all the alum, that 100 gr. of sulphate of potash could produce. The mixture having occasioned only a slight precipitate of alum, I concluded, that the whole of the sulphate of potash had entered into the composition of the 524 gr. of alum obtained.

Quantity produced with a

Exp. 2. On the supposition, that sulphate of potash contains 62 per cent of potash, I saturated 62 gr. of potash,

* The proportions being all that is of importance, it would be superfluous to reduce the rest of the quantities: but this is given, to mark the quantity operated on. C.

purified

given weight of potash and of sulphuric acid.

purified with alcohol, with 48 gr. of sulphuric acid at 66° [sp. gr. 1·848]. I then mixed this sulphate with 850 gr. of solution of sulphate of alumine at 34°, and conducted the rest of the process as in the preceding experiment.

But what was my surprise, when, on adding together all the alum produced, I found but 408 gr., instead of 524, which the former experiment had yielded. The comparative results of these two experiments, which I varied with quantites alternately greater and less of sulphate of potash and sulphate of alumine, demonstrated to me, that the proportions of potash and acid contained in the sulphate of potash were very different from those hitherto laid down. In fact, knowing how much alum is produced by 100 gr. of sulphate of potash, and how much may be obtained with a given quantity of potash saturated afterward with acid, it was easy for me, on comparing the results of these two experiments, to ascertain by calculation the respective proportions of acid and base, that enter into the composition both of sulphate of potash and of alum.

Proportions of acid and base in sulphate of potash.

For example, since with 100 gr. of sulphate of potash I obtained 524 gr. of alum; and on the other hand, 62 gr. of potash gave but 408; I necessarily concluded, that the potash contained in 100 parts of sulphate of potash must make four fifths of its weight. But reflecting, that, on the one hand, this quantity of potash was much greater, than is generally admitted in the sulphate of potash; and, on the other, that the acid could not lose two thirds of its weight in this combination: I could not but suspect, that the potash contributed to this loss in a certain proportion, and hence sought some means of ascertaining the quantity of water it might contain. Accordingly I made a great number of experiments with this view, and with the result of which I have so much the more reason to be satisfied, as the question to be solved is very important; since even at present, while it is allowed, that potash purified by alcohol contains water, the quantity is not agreed on: for Mr. Berthollet, according to recent experiments, admits only 15 per cent, while Mr. Darcet finds twice this quantity by his*.

Water in potash.

* See Journ. vol. XXVII, p. 31.

Hence

Hence I have presumed, that to make known the result of my experiments, though undertaken with other views, might be of some advantage. In fact, finding by synthesis the quantities of potash; and of acid entering into the composition of sulphate of potash and having afterward ascertained, how much water they lose respectively in this combination; it appears to me, that the question is solved a priori. I must confess, however, that some difficulties occurred at first in ascertaining the quantity of water contained in potash; difficulties which have afforded me an opportunity of knowing, that, interesting as the experiment of Mr. Berthollet is, the treatment of potash with iron filings is not a method sufficiently precise to be conclusive. My opinion on the contrary was, as it still is, that the substances most proper for detecting the water contained in potash should not be oxidable; and that their action should be confined to the separation of the water contained in the potash. Among the experiments I made, the following appeared to me best to fulfil the conditions I had imposed on myself. Attempts to ascertain this.

Exp. 3. Twenty grammes of potash prepared in the laboratory of Mr. Vauquelin were carefully mixed with 160 of very pure silex, which must have been dry, as it was heated for two hours in a forge fire before it was used. The mixture was introduced with much caution into a glass tube about 2 cent. [7·87 lines] in diameter. This tube, one of the extremities of which was closed, weighed 72 gr., and with the mixture 252; very good weight, it is true, but this excess I ascribed to moisture attracted by the potash during the trituration. This tube I introduced into a small cylinder of sheet iron, to prevent its being fused by the direct action of the fire. This apparatus was subjected for an hour to the action of a very moderate fire. No sooner did the mixture receive the impression of the heat, than a very large quantity of water reduced to vapour was suddenly expelled, and continued to be evolved five or six minutes, after which nothing more was extricated. Experiment.

When the tube was cold, I weighed it very carefully, and found it had lost 5·5 gr. This experiment, which I repeated several times, sometimes collecting the water, constantly afforded me the same results, both with potash of my own preparing, and with that from the laboratory of Mr. Vauquelin; whence 27·5 of water in 100 of potash.

Proportions of the sulphate.

whence I conclude, that in 100 parts of potash purified by alcohol there are 27·5 of water; and, setting out with this datum, that the potash in 100 parts of sulphate of potash is 57·71, instead of 52 as assigned by Bergman.

I cannot omit remarking however, that the analysis of alums by Mr. Vauquelin* demonstrates the presence of sulphate of potash in them nearly in the same proportion, as appears from synthesis: a result showing the confidence to be placed in the analyses of that learned chemist, and leaving us to regret, that he relied on Beryman for the proportions of acid and base in the sulphate of potash.

Experiment to find the proportions of acid and base in nitrate of potash.

Exp. 4. Desirous of knowing the proportions of acid and base in nitrate of potash, I dissolved by the assistance of heat 100 gr of very dry nitrate of potash in 800 of a solution of sulphate of alumine at 34 [sp. gr. 1·307.] After the liquor was cold, I obtained 376 gr. of alum. The mother water was set to evaporate again, but as it crystallized confusedly I added 10 gr. of sulphuric acid at 66° [sp. gr. 1·848], because experience had taught me, that whenever such a solution contained an acid foreign to the alum, an excess of sulphuric acid was necessary to promote the crystallization of the alum. In fact, as soon as this mixture was made, a considerable precipitate took place, which, after being drained and dried, weighed 84 gr. Lastly to satisfy myself whether the mother water still contained alum, I added anew 100 gr. of the solution of sulphate of alumine. This addition, increasing the density of the liquid, favoured the precipitation of the small quantity of alum, which it still held in solution. When this last product was drained and dried, it amounted to 2 gr.; which, with what was obtained before, made 462 gr. of alum. As it had crystallized however in a liquid containing principles foreign to its composition, it became necessary to purify it. With this view I dissolved it, and crystallized afresh. From this process, I obtained only 452 gr. of alum, but certainly very pure.

49·76 potash, and 50·24 nitric acid.

This experiment, which I have repeated several times, and with different quantities, always gave me results confirming the former: whence I conclude, that if 100 gr. of nitrate of potash produce 452 gr. of alum, 49.76 of

* See Journal, 4to series, vol. i, p 318.

potash

potash, and 50·24 of acid, must enter into the composition of 100 parts of the nitrate.

Exp. 5. The object of this, as of the former experiment, was to ascertain, whether the base and acid in muriate of potash were in the proportions commonly admitted. For this purpose I employed the means I have just described; and, as it would be superfluous to repeat the particulars, I shall confine myself to the results.

Proportions of muriate of potash.

100 gr. of dry muriate of potash, treated as in the preceding experiment, produced 607 gr. of crude alum; which, after being refined, left but 592 gr.: a result proving incontestably, that 100 parts of muriate of potash contain 65·17 of base and 34·83 of acid. This experiment, which, like the preceding, was several times repeated, always afforded me similar results.

65·17 base and 34·83 acid.

From the experiments that have been described, it follows,

General conclusions.

1. That 100 parts of sulphate of potash contain 57·71 of potash and 42·29 of acid, which, from the state of concentration in which it exists in this sulphate, are equivalent to 60 parts at 66° [sp. gr. 1·848].

2. That to form 100 parts of alum requires 42·77 of sulphuric acid at 66°, instead of 30 or 31, the quantity generally admitted; 11·01 of potash; and 10·50 of alumine: a quantity equal to what was found by Vauquelin.

3. That 100 parts of very pure alum contain 19·08 of sulphate of potash, 30·92 of sulphate of alumine, and 50 of water of crystallization.

4. That 49·76 parts of potash and 50·24 of acid enter into the composition of nitrate of potash.

5. That 100 parts of muriate of potash are composed of 65·17 potash, and 34·83 muriatic acid.

6. That it is certain potash purified with alcohol contains more than a fourth of its weight of water, since, from the experiments that have been related, 27·5 per cent may be obtained from it.

7. Lastly, that by means of sulphate of alumine, with the simple base and crystallized, we may in future, in the analysis of the substances of either of the three kingdoms, detect the smallest quantity of potash contained in either:

the

the method admitting of great accuracy, since the product, from which the proportion is ascertained, weighs in the proportion of 9·08 to one of dry potash.

XVII.

Analyses of Minerals; by MARTIN HENRY KLAPROTH, *Ph. D. &c.*

(Continued from vol. XXXI, p. 382.)

Triple sulphuret of lead from Cornwall.

ORE of antimony and lead from Nanslo, in Cornwall*.

Lead	39
Antimony	28·5
Copper	13·5
Sulphur	16
Iron	1
Loss	2
	100

Sulphuret of bismuth and copper.

Ore of copper and bismuth from Wittichen.

Bismuth	47·24
Copper	34·66
Sulphur	12·58
Loss	5·52
	100

Meteorolite of Agrau.

Native iron of a meteorolite from Agrau.

Native iron	96·5
Metallic nickel	3·5
	100

and Mexico.

Proust analysed a meteorolite from the province of Chaca Gualamba, in Mexico, sent by Rubin de Celis, and found in it native iron and metallic nickel.

* See Journal, vol. IX, p. 14; XX, p. 332; and XXIV, pp. 225, 251, 321; for a full account of the triple sulphuret of lead, copper, and antimony, from Cornwall, by Mr. Hatchett, Mr. Smithson, and count de Bournon.

Humboldt

Humboldt brought over a meteorolite from the province of Durango, in Mexico, from which prof. Klaproth obtained Another.

Native iron	96·75
Metallic nickel	3·25
	100

Native iron from the iron-mine of St. John, near Grosskansdorf, in Saxony. Native iron from Saxony.

Iron	92·5
Lead	6
Copper	1·5
	100

Sparry iron stone from Dankerode, in the country of Halberstadt. Iron spar fro Dankerode and

Black oxidulated iron	57·50
Oxide of manganese	3·50
Calcareous earth	1·25
Carbonic acid	36
	98·25

Sparry ironstone from Bayreuth. Bayreuth.

Black oxidulated iron	58
Oxide of manganese	4·25
Magnesia	0·75
Lime	0·50
Carbonic acid	35
	98·5

Blue iron earth from Eckartzberg (native Prussian blue of Bergman and some other authors). Phosphate of iron.

Oxidulated iron	47·5
Phosphoric acid	32
Water	20
	99·5

(To be continued).

SCIENTIFIC NEWS.

Royal Medical Society of Edinburgh,

Prize question. ALL members of the Society are invited to write an *Experimental Essay* on the following subject.

"To determine by experiment what substances are ex-
"haled by the skin; and the changes, if any, which they
"produce on the surrounding air".

The dissertations are to be written in English, Latin, or French, and are to be delivered to the secretary on or before the 1st of December, 1813, (being the year succeeding that in which the subject is proposed). The adjudication will take place in the last week of February following.

To each dissertation shall be prefixed a motto, and this motto is to be written on the outside of a sealed packet containing the name and address of the author. No dissertation will be received with the author's name affixed, and all dissertations, except the successful one, will be returned, if desired, with the sealed packet unopened.

Geological Society, April 3, 1812.

Geology of the coast of Labrador. A notice relative to the geology of the coast of Labrador by the Rev. Mr. Steinhauer was read. The only accounts, which have hitherto been published, concerning this part of the British dominions are the memoirs of Mr. (afterward sir Roger) Curtis, inserted in the Philosophical Transactions, and Mr. Cartwright's Journal.

Moravian missions there. The Moravian missionaries in 1772 established in this country their first settlement, called Nain, in lat. 56° 38′; and subsequently Okkak, in lat. 58° 43′, and Hopedale, in 55° 36′. In the course of last year they doubled cape Chudleigh, in lat. 60° 20′, and descended on the western side of the same promontory as far as lat. 58° 36′. The leisure of the missionaries, when opportunities occur, is employed in collecting materials for a Natural History of the country, they have kept tables of the thermometrical and barometrical variations, have procured specimens of most of the native vegetable

vegetable productions, and have from time to time sent over specimens of such minerals as attracted their notice.

The general aspect of this dreary region is that of bare and barren rock, towering in craggy eminences; and of sandy marshes, on which are formed a few pines, brushwood, and aquatic mosses. In several parts of the country the rocks are intersected by chasms, running generally in a right line to a considerable distance, which, when covered with snow, form dangerous pitfalls. The highest mountains extend along the eastern coast: the elevation of one of them, called Mount Thoresby, has been ascertained by actual measurement to equal 2733 feet, and a few others probably attain the height of 3000 feet. Face of the country.

From the islands near cape Chudleigh the missionaries have sent specimens of large-grained pale granite with garnets. The island of Ammitok, in lat. 59° 20′, consists entirely of a crumbling garnet rock, in which hornblende sometimes occurs. The mountains about Nachwak bay furnish lapis ollaris. Minerals.

On the south of the high land of Kiglapyed, in lat. 57°, the district commences where the Labrador felspar is found; this mineral occurs not only in rolled stones on the shore, but in spots in the rocks in the neighbourhood of Nain, and particularly in the rocks bordering a lagoon about 60 miles inland, in which Nain North river terminates. The same district also produces the hyperstène or Labrador hornblende.

At Hopedale a limestone occurs, from which have been procured specimens of reddish limestone, of calcareous spar, and of a variety of schiefes spar.

The country to the west of cape Chudleigh, as far as it has been explored, is called the Ungava, and abounds with red jasper, with hæmatites, and with iron pyrites.

April the 17*th*, 1812.

An account of the brine springs at Droitwich, by Leonard Horner, Esq., Sec. G. S., was read. Brine springs at Droitwich.

The town of Droitwich has been noted for the manufacture of salt during at least a thousand years, but no detailed account has hitherto been published of the natural and chemical history of the brine springs, from which it is procured.

cured. The brine pits are in the centre of the town, being situate in a narrow valley, through which the small river Salwark flows. The prevailing rock about Droitwich is a fine grained calcareo-argillaceous sandstone of a brownish red colour, with occasional spots and patches of a greenish blue. At Doder hill, in the immediate vicinity of the salt pits, the rock appears to be a stratified sandstone of a greenish gray colour, and more indurated than the red rock. It also differs from this last in containing slender veins of gypsum.

Strata covering the salt.

No new brine pits have been sunk for the last thirty years: the only particulars therefore concerning the strata covering the salt, which Mr Horner has been able to obtain, are derived from Dr. Nash's History of Worcestershire, and from an inhabitant of Droitwich, who was on the spot when the last pit was sunk. From these authorities it appears, that the depth of from 35 to 45 feet below the surface is occupied by beds of gravel, of red marly clay, and of blue and white stone. To these succeeds a bed of gypsum about 105 feet in thickness, immediately below which is what is

River of salt.

called the *river of salt*, which is a stratum of nearly saturated brine, 22 inches in depth, lying on a bed of rock salt, the thickness of which is unknown, no borings having been sunk in it to a greater depth than five or six feet.

Construction of the pits.

In constructing the pits, the method is to sink a shaft about eight feet square into the gypsum, and then to pierce this bed by a borer four inches in diameter: the borer is known to have passed through the gypsum by its suddenly dropping 22 inches, the depth of the river of salt. As soon as the borer is withdrawn, the brine suddenly rushes up, and overflows

Produce.

at the mouth of the pit. There are only four pits at present in all, and the annual quantity of salt which they afford is about 1600 tuns.

The brine.

The brine from all the pits is perfectly limpid, and when in a large body has a pale greenish hue, similar to that of sea-water. To the taste it is intensely saline, but without any degree of bitterness. The specific gravity differs in the different pits, probably on account of the greater or less accuracy with which the landsprings are stopped out: that of perfectly saturated brine is equal to 1210·39 (water being 1000): that

that of the five pits examined by Mr. Horner was found to vary from 1206·11 to 1174·71; and on evaporation afforded from 2289·75 grs to 1922·97 grs of entire salt, dried at 180° Fahr., in a pint.

This salt, from a careful analysis, appears to be composed of — The salt.

96·48	Muriate of soda
1·63	Sulphate of lime
1·82	Sulphate of soda
0·07	Muriate of magnesia
100·00	

Droitwich and Cheshire brinesprings compared.

On comparing the brine of Droitwich with that of Cheshire, as described by Mr. Holland in his Agricultural Survey of Cheshire, and by Dr. Henry in his paper on the subject in the Philos. Trans., it appears, that the strength of the different brines is nearly the same, that the Cheshire brine contains rather a larger proportion of muriate of soda, that the Droitwich brine is free from carbonate of lime, oxide of iron, and muriate of lime, all of which are found in the Cheshire brine; and finally, that the latter is free from the sulphate of soda, which is contained in the former.

Delineations of meteorolites.

Mr. Sowerby, author of British Mineralogy, has just pub-published a plate representing the meteor-stone, which was seen to fall in Yorkshire, on the 13th of December, 1795, accompanied by engravings of part of the one which fell in Scotland in 1804, and of that which fell in Ireland in 1810, all of which are deposited in his museum.

Animacules not produced from eggs.

Mr. J. B. Fray, member of the legion of honour, and of several literary societies, has lately published some experiments which he made, to prove, that animalcules of infusion are not produced from the eggs of invisible insects floating in the atmosphere.

Glass globe filled with distilled water.

On the 8th of nivose, 5, [dec. the 28th, 1796], he took a glass globe, that would hold about six common bottles of liquid; well rinsed it with distilled water; and then filled it with water, which he had just distilled a second time. Having inverted it on a pneumatic trough, he expelled about

and a mixture of oxigen and hidrogen; about five sixths of the water by introducing first a portion of oxigen gas, and then three times as much hidrogen gas. stopped close, He then corked the globe in the trough with a cork that fitted it very tight; and tied a piece of wet bladder over the cork as soon as it was taken out. When this bladder was dry he covered it with putty, and tied another piece and placed on a hot bed. of bladder over this. The globe thus prepared he placed in very hot dung, into which it was sunk to the level of the water, and covered it with a frame. On the 9th of january he removed the frame, and examined the globe. No alteration was perceptible. On the 24th it was in the same state; but, the heat of the bed having diminished, some fresh hot dung was added; care being taken, to agitate the globe as little as possible in moving it. On the 8th of february the water appeared not perfectly limpid; but no pellicle, or distinct substance of any kind, was perceptible. In 8 weeks a green vegetation appeared, On the 26th, as soon as the frame was removed, the water appeared greenish. On a closer inspection long vegetations of a beautiful green colour were perceived ramifying in all directions on the bottom and sides of the vessel. Several, that were of a larger size, but less green, were suspended in the water, and had a mucous appearance. Mr. Fray now removed the globe to the window of his study. On the 1st of march he opened the window, the sun then shining on the globe, and perceived here and and soon after animalcules. there on the summits of the vegetations, little insects, moving about pretty quickly. He counted ninety six, moving in various directions. They were all of the same species; and he soon discovered, that they were of the genus podura. For a few days their number increased, and they were more brisk in their motions: but in about three weeks, or less, their motion had ceased; and they were dead. Their bodies soon changed colour, and became of a whitish gray.

The organic matter at length decomposed. As soon as the sun had acquired some power, the green matter gradually grew pale. At length it disappeared entirely, its filaments were decomposed, and all this organic and vegetable matter was precipitated to the bottom of the water, where it formed a very white mucous sediment. After some months the surface of the water was covered

covered pretty copiously with an oily matter. On opening the globe a slight smell of mouldiness was perceived.

Appearance of the water in this state.

A drop of the water, with a little of the pellicle swimming on it, being taken up with the point of a toothpick, and examined with the microscope, it exhibited an immense number of globules, of various sizes, almost all motionless: in every drop of water however one or two were perceived, that had a very slow motion.

Mr. Fray made several experiments of a similar kind, all of which afforded curious results, and he intended to continue them.

Application of oximuriatic acid against miasmata.

In the autumn of 1810, Messrs. Thenard and Cluzel were sent to Flushing, to direct such means for preserving the health of the persons exposed to the dangers of that insalubrious situation, as they might think fit. They ascribe a great deal of benefit to the following practice. In the apartments for the soldiers, as well as those where prisoners were confined, they placed earthen vessels filled with oximuriatic acid greatly diluted with water; and they obliged every man employed on the fortifications, to dip his hands into one of the vessels every morning before he went out to his work. They placed similar vessels in the ditches of stinking mud: so that from these, and the fumigations employed, the workmen were immersed day and night in an atmosphere of oximuriatic acid.

A remedy for the itch.

As many topical remedies for the itch were presumed to act by means of the oxigen they contained, it had been supposed, that oximuriatic acid would answer the purpose: and it appeared, that many of the prisoners infected with the itch soon experienced the good effects of this immersion of their hands in dilute oximuriatic acid*. One, who had the disease all over him in an inveterate degree, that had resisted every application, requested permission to wet rags in the bowls, and rub his body with them; and by these means was perfectly cured in a few days.

Given internally in putrid fever,

At Carcassone, we are told, upward of four thousand Spanish prisoners being attacked with fevers, "adynamico-

* From this passage it appears, that the French employed their prisoners on the works. C.

ataxic

ataxic in the highest degree, as if they had been inoculated with gangrene," Dr. Estribaud, finding fumigations with oximuriatic acid of little effect, in places so crowded with the sick, except as a preservative, administered the acid internally with the greatest success. He mixed six or eight drachms of the acid with a quart of a mucilaginous decoction; but it is not said in what dose he administered the medicine. He asserts, that its efficacy might be compared with that of bark in intermittents.

and to prevent hydrophobia.

We are further told, that the acid has been administered internally, in the hospital at Bordeaux, to several persons bitten by a mad wolf; and that the hydrophobia was prevented by it.

Securing corn from the weevil.

Mr. Sonnini informs us, on the authority of a German Journal, the title of which he does not give, that if a granary be swept clean from every grain of corn, so as to leave no food for weevils, and hay be then kept in it for six months, corn may afterward be placed in it safely, without any danger from these destructive insects.

Remedy against leaf lice.

Mr. Braun, of Vienna, gives the following as a cheap and easy mode of freeing plants from leaf-lice. Mix an ounce of flowers of sulphur with a bushel of sawdust, scatter this over the plants infested with these insects, and they will soon be freed from them. If they should appear again, the process may be repeated.

Medical and Chemical Lectures.

Medical and chemical lectures.

On Monday, the 1st of June, as usual, at No. 9, George Street, Hanover Square, the courses of Medical Lectures will recommence at eight o'clock, and the Chemical at a quarter after nine. By George Pearson, M. D., F. R. S., sen. Physician of St. George's Hospital, of the College of Physicians, &c.

A register is kept of the cases of Dr. Pearson's patients in St. George's Hospital, and a clinical lecture is given on them every Saturday morning, at nine o'clock.

A

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AND

THE ARTS.

JULY, 1812.

ARTICLE I.

On the Dissection of Flowers. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

I Have long reproached myself with not again bringing this subject before the public, as one of the most important in physiology, and that which must prove most absolutely the existence of the line of life. One of the first facts I endeavoured to show in my early letters, was, that every flower was formed by a part of the stalk appropriated to it; and that Linneus was, as usual, most absolutely right, when he advanced, that the wood formed the stamen, the inner bark the corolla, and so on to the rest of the division. The present letter should have preceded many you have received and published, as it will I hope not only explain how the mechanical work is concealed in a plant, but illustrate the fact just mentioned; enabling any person (if so inclined) to follow me in my dissections, and teach them how to seek the mechanism that belongs to each separate part. All vegetable structure is formed in one peculiar manner, that is, cylinder within cylinder; and on this curious

How to find the mechanism of plants.

curious construction most of its mechanical contrivance depends. It is strange we should annex such extreme simplicity to the vegetable form, when mineralogy is hourly presenting us with a variety of curious and difficult figures, such as to puzzle the first mathematicians to find a suitable name for their multangular solids; or a more simple derivative from which to trace their integral crystallizations. But with respect to botany the time is now come, I hope, when its mechanism will be too well known, not to show the fallacy of these ideas: for if inanimate mattter requires or can be resolved into such complex forms, how much more where motion makes mechanism constantly requisite to supersede volition; and make amends for every assistance this would bestow?

This mechanism an important study.

Each day's work in dissection more and more proves to me, that the mechanism of botany is an important science; which would develope to us, if known, the most wonderful proceedings in nature; and give us more exact notions of the sort of existence of plants (independant of volition and wholly governed by mechanical powers) than we now possess, and that the simplicity we talk of so much is merely that found in all nature; "the labour of the means never surpassing what the necessity of the end absolutely requires": of which however we are not always proper judges; for so various is the motion, so complicated the effect, to be produced, that it is impossible to dissect a single plant, and not observe some mechanical wonder, that makes one feel how little is understood of the purposes, for which it is intended; and most ardently long to attain that knowledge which to gain is now become the labour of my life. But it is in the whole general system of physiology, that that beautiful simplicity is observable, not in the mechanical part. There indeed it is unequalled: and I hope, when I come to review the whole of the present work, from the immense number of drawings I now possess, I shall prove it most exquisite.

All vegetables formed cylinder within cylinder.

By means of this curious construction of cylinder within cylinder, formed of each different sort of matter; the vessels belonging to each circle; and the juices appropriated to each vessel, can never in the smallest degree interfere with each

each other. Suppose seven or eight glass cylinders placed within one another, and having ribs of the same matter, which convey the juices of each to their appointed place; would it not be most easily understood, that the liquid thus carried, and the mechanism thus enclosed, can in no manner disturb those in the adjoining circles, though certainly increasing the size of the whole? This is exactly the case in the vegetable structure: as for example; when the mechanism is to be sought that governs the leaf; draw off the rind of the plant, and in the next matter, (that is on the bark) will be found the whole of what forms and regulates the motion of the leaves. Great care must be taken however not to carry off the balls with the rind; but, if properly stripped, the whole management of the spiral wire will then be discovered in regular order, with the balls on which it is wound, and the knots by which it is fastened.

To find the mechanism of a flower.

If the mechanism of the flower is desired, the rind is first drawn down, and displays the mechanism of the calyx, with the partial skin that leads up the vessels to its edge, and generally lines it. When this is thoroughly examined, it must be taken off with the greatest care; and it will display a green matter of a thicker kind, which is the skin on which the vessels of the corolla repose; this regular cylinder reaches up to the claw of each petal, and gives to it the vessels that are to meander through it, and the juices that inflate them. This, when properly viewed, must be cut off with a lancet, and a yellow and also a thin white skin will appear next, which hold the vessels of the stamen between them, and convey them either to the filaments or the corolla; in which they perform the rest of their journey, as in the primula; or in a skin that forms an additional cuticle to the pistil, as in the malva; or in a cylinder that stands up round the female, as in many flowers. But let it pass where it will, it always has a skin appropriated to the stamen alone; till it reaches the part where the corolla branches off; and afterward it has no connexion with the juices of the petals, though lying on them; or with the pistil, though enclosing it; as I shall now show by the dissection of a flower: I chose the peach out of several hundreds drawn in the same manner, because it is now in

 season,

season, and I could review my sketch; but all flowers in this respect are the same.

How to dissect the flower.

I shall first dissect the flower by removing skin from skin as the easiest method of making it thoroughly comprehended. I shall in the second place cut the flower down the middle; halving the pistil; by which means the interior, with the vessels which run up to each part, must be *exactly displayed*: and lastly I shall give you a vegetable cutting of the flower, just where the vessels *divide* under it, and run, up the bark to form the calyx, the inner bark to form the corolla, the wood to form the stamen, and the line of life to complete the pistil: and this will, I hope, make the description so plain, that I shall not again be obliged to return to the subject.

Explanation of the plate.

Plate IV, fig. 1, is the bud of the peach. I first remove the scales A A, which generally go on shooting as long as the severe weather continues. I then with great ease remove the calyx. It is seldom possible to get it off whole, as it must be removed without displacing the corolla; which is difficult to do: but custom soon teaches the way. The calyx, when taken off, is seen at fig. 2: BB are the reservoirs of a glutinous liquid, resembling the juices of the bark, which appear, by varnishing the exterior, to defend it from the attack of vermin; which from its delicacy would otherwise cause it to become a complete victim. This part has no connexion with the nectary, which I shall not attempt to point out in this letter, but keep that part for a separate paper, which it well deserves. CC are the vessels which run down till branching off to the bark. Fig. 3 is next taken off. It is a green skin belonging to the inner bark; which is fastened to the corolla, and conveys the regular vessels DD from the inner bark of the stem to each separate petal. Within these vessels (as I have before observed) are the juices of the inner bark, and the spiral wires which are thus carried up to perform their office of opening and shutting the flower. Next to this is fig. 4, which is a very thin skin of white; and a very thick one of yellow matter between which are concealed the vessels which convey the juices that form the pollen, and carry it first to the filaments, and thence to the anthers. Thus they are most plainly three separate cylinders, capable of being divided, and placed together again;

fig. 4.

fig. 4 laid within fig. 3, and 4 and 3 within fig. 2: the line of life, and pistil alone being taken out, which are seen at fig. 5. When these are replaced the flower is perfect, and has been regularly dissected, as I promised, skin from skin; and between each cylinder the mechanism is concealed, that belongs to each division.

All flowers dissected nearly the same.

Of the many hundreds (I might say thousands) of flowers dissected in this manner, of every class and order, I never yet found one that did not admit of this arrangement. The gynandria tribe is exactly the same with respect to its cylinders, which are always to be taken off in progressive order; and let the stamens appear where they will in the flowers, their vessels always pass up in this manner, whether afterward bound to the corolla, the calyx, or the pistil.

Perpendicular section of the flower through the middle.

I shall now show the flower when divided into two halves, and cut perpendicularly down the middle. I have magnified this much, in order to show how completely the several parts are appropriated; and how separate the line of life and pistil are from every other division, till they join the stalk. At fig. 6 are three buds thus cut, without their corollas or calyxes, but having their own peculiar cylinders, which reach up to them. All within the points and the letter E is the pistil belonging to each flower, with the line of life running up to each pistil; which in the stem bounds the pith, till it stops, and then runs up to form the female. It may be seen dividing the seed at F, and halving the corculum. G is the interior of the flower. H I K are the three cylinders of the stamen, corolla, and calyx; (at least the skins to which those parts are fastened, and which conveys the vessels or mechanism up to them;) and L is the rind. I have not properly proportioned the thickness of the cylinders; as I feared they would not be seen; nature requires so thin a skin, to which it will fasten and adapt such powerful mechanism, and such a quantity of vessels, that it requires a long practice in dissection before we can give credit to our sight in this respect. I have shown several buds starting from the line of life, at T T T; and at *u* will be seen how the vessels arrange themselves, to enter the different parts of the stalk to which they belong. Fig. 7 is now my last dissection, it is a horizontal cutting of the part taken from fig. 6 at M, to

to show in a still plainer manner how each cylinder runs up to form its appropriate part; in the flower O is the bark separated to form the calyx; P the inner bark to form the carolla; Q the wood to convey the nourishment to the stamen; and the whole interior between the points, belonging to the pistil and seeds at R; the line of life bounding the part as well as leading up the middle. The stamens are perhaps better marked where there are fewer, I have given two very good dissections of this sort of cutting of the bottom of a flower in my third letter (see Journal, vol. XXIII Pl. IX, p. 350.) I have said, that it signifies little whether the stamen, when once past the cylinder, proceeds up the calyx, corolla, or pistil, since it has equally its peculiar vessels. Sometimes the stamens are on one side of a flower only; and then the cylinder, instead of passing in equal thickness all round; is found large only on one side; this is the case in the cutting of the violet, but it is then even more distinguishable. Often in dissecting, you find the stem suddenly enlarged; on cutting it through the middle, the pith is found still of the same size, the line of life at the same distance, and the wood not altered; but the part between the rind and bark extremely increases. When this is the case, you may be sure that it is the mechanism belonging to the leaves, or that it is a stem that turns on a ball, as in the arenarias, stellarias, and galiums; but it generally denotes the mechanism of the leaves, which is seen if the rind is drawn down. If the size appears enlarged in the wood, it is always the buds which cause it, and they will be found in numbers starting from the line of life: (as seen at T fig. 6, or at T fig. 8.)

Laburnum dissected.

I shall only add to this letter a branch of laburnum, Plate V, fig. 1; with a section greatly magnified, at fig. 2; in which are shown the flowers just shooting at V; and the line of life passing up as usual to form the pistil. As all the other parts are extremely small in proportion, they are not much marked in this sketch, which, however, very evidently shows the new wood, which is always generated for the use of the flower buds within the boundaries of the line of life at SS, and the little line of old wood which runs next the line W. It is impossible, not to see how exactly each bud shoots from the line of life, and how wholly separate the bark *y* is from the rest,

rest, and how entirely the new wood or albumen x divides it from the wood; it is only to recollect that these are each of them regular cylinders of different degrees of thickness, having their vessels closely applied on each part, as well as the mechanism appertaining to them, adhesive to every circle; and it will be easily understood how such delicate and complicated machinery can be each in its separate cylinder, without interfering or disturbing each other.

I explained in my last letter, that it was by this means, and by a perfect knowledge of the dissection of plants, that I was able to trace the male and female in the cryptogamian class. I have by the same analogy proved which are the most important lines and vessels to the vegetable kingdom. I did not set up to form a system; it is dissection alone that has created it for me; I trusted to the strict conformity of nature, and never attempted to make one part agree with the other, but drew the sketch exactly as nature presented it to me. Yet on looking back, I find they all agree; the same conformity is maintained throughout. Is not this the most convincing proof of the exactness and truth of every part? In my next I hope to give a view of the manner in which the buds shoot in annuals from the stalk; as it is really so curious and beautiful, it is well worth a letter to itself. I have also promised one on parasite plants, a very amusing and also important subject: matter presses on me so much at this time of the year, I cannot draw quick enough to keep pace with it. Uniformity of nature.

I am, SIR,

Your obliged servant,

AGNES IBBETSON.

I shall add a few lines to mark the constant use of the screw in all plants, it serves a double purpose: first, that of covering and concealing the buds; and next, the easily dividing each circle to let them out when ripe. If such a specimen as fig. 9 is taken, and the rind and bark stripped off, it will appear as at fig. 10; with the wood separating into threads to let the buds pass and to make a hollow way for them. It appears to me, that there requires no other proof than

than this to show, that the buds proceed from the interior, and therefore from the line of life, for nothing can be more different than the appearance of this specimen, and of the wood in the stem, which is perfectly straight, and without any openings, while these appear in such quantities in fig. 3. Almost every tree in the spring shoots its buds in the same way, but few in such numbers as the laburnum.

II.

Remarks on the Perforations made in Paper by Electrical Batteries. In a letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

An experiment in favour of two electric fluids stated.

MANY philosophers are of opinion, that the phenomena of electricity and galvanism are caused by the cooperation of two distinct kinds of subtile matter; which they denominate the positive and the negative electric fluids. Amongst many other facts and argument in favour of this hypothesis, the following experiments formerly appeared to me as amounting to a proof; because it seemed to be little short of a mechanical demonstration. If a quire of writing paper be placed betwixt the points of two metal rods, which are in contact with the opposite sides of it, and the charge of an electrical battery be transmitted through the wires, the bundle of paper will be perforated in the direction of a right line joining their points; and each orifice of the perforation will be surrounded externally by a bur, or prominent rim. The peculiarity of this experiment consists in the two rings or elevated borders, which are driven outward in opposite directions by the force of the discharge; and their presence is supposed to prove the existence of two opposite currents; which strike the parallel sides of the quire at the same instant, and meet in the middle of the paper, after perforating the sheets in contrary directions.

This experiment called in question and defended.

The preceding experiment happened to be the subject of conversation in a company, where I had the pleasure of meeting Mr. Webster; who lately gave a course of lectures at Kendal

Kendal in his progress northward to Edinburgh and Glasgow This gentleman observed, that he had reason to suspect the accuracy of the foregoing statement; for, when he undertook to perforate a slip of card paper by an electrical discharge, he invariably found but one bur, and this appeared on the side of the card, which was connected with the negative surface of the battery. In consequence of this remark, the gentleman was asked, if the appearance was the same when the discharge was made from the negative to the positive side of the battery, as well as when it passed in the opposite direction, namely from the positive to the negative side.

To this question Mr. Webster replied ingenuously, that he had always made the experiments in the latter manner; and my predilection for the idea of a double current induced me to obviate, or at least to weaken the objection, by remarking, that, the positive current being put in motion before the negative fluid, it acquired a preponderance, which enabled it to drive the paper in the direction of its own course, and consequently to raise a single bur, on the side of the card that was connected with the negative surface of the battery. I moreover observed in addition to the last remark; that, if the preceding reply to Mr. Webster's objection had truth for its foundation, the place of the bur might be removed to the contrary side of the card by inverting the experiment, so as to give a preponderance to the negative current; which would then drive the paper before it, and form an elevated rim on that face of the slip, which was connected with the positive surface of the battery.

The method proposed for repeating the experiment.

The want of facts, which is apparent in this discussion, determined me to repeat the experiment with the variations and under the conditions, that had been prescribed by myself. For this purpose I procured several slips of card paper; that were cut accurately into the shape of right angled parallelograms; and all of them had both their faces divided diagonally, each by two diameters intersecting in the centre of the plane. Pieces of tinfoil were then reduced to the figure and size of the triangles, which had the shorter sides of the parallelograms for their bases. One triangle in each side of a slip, was covered in the next place by one of these metallic coatings; the pieces of tinfoil being so disposed as

to

to make their bases coincide with the oposite ends of the card, while their points fell upon the centre of the surface to which they were pasted. This arrangement evidently formed an intercepted conductor; which obliged the electric charge to pass through the card paper in a right line perpendicular to its oposite faces. Perhaps I may be blamed for giving a circumstantial description of a very simple contrivance; but minuteness always appears to me absolutely necessary in relating the manner of conducting an experiment.

General result not favourable to a double current.

I made the discharge from the positive to the negative side of the battery in my first trial; in consequence of which two burs were raised at the centre of the card; namely, a small one on the face connected with the positive coating, which seems to have escaped Mr. Webster's notice, and a second on the opposite side of the paper, to which he directed his attention exclusively. This perforation bore a strict resemblance to the holes that a punch makes in a plate of metal, or other ductile substance; for I found upon trial, when an instrument of this description was driven forcibly through a card placed on a piece of soft wood, or through a plate of lead fixed by nails over a hole, the perforation made by it was furnished with two burs, like those produced by the stroke of a battery. The prominent ring surrounding the upper orifice, where the operation of the punch began, was small; but the rim on the opposite or under surface of the card or lead was comparatively large. The reason of this difference is too manifest to require an explanation; but the strict analogy observable in the two experiments with the battery and punch led me to attribute the perforation in the former case to the action of the positive current alone. When the experiment was inverted, and the discharge made from the negative to the positive coating of the jars, no alteration was produced; for the minute bur still kept its place upon that side of the card which was connected with the positive surface of the battery; and the large bur was formed on the opposite side of the paper.

The existence of a negative fluid is not demonstrated by

This result shows the futility of my remarks on Mr. Webster's objection to a double current; for, if the positive current produced the perforation in my first experiment

periment, the same cause undoubtedly produced the same effect in the second case. This conclusion leads to another of still greater importance; for, if the perforations in question are invariably made by the positive current, the experiment under consideration affords no mechanical evidence, demonstrating the existence of a negative fluid. It will not be improper or superfluous to conclude the present letter by observing, that I made similar trials with several slips of writing paper, which were pasted together by their ends and coated with tin-foil like the cards. The result in this case was always the same; for the less bur was on the side of the bundle which was connected with the positive surface of the battery; and when the slips of paper were separated, the larger bur of each piece pointed to that face of the bundle which communicated with the negative coating of the jars.

the experiment.

MIDDLESHAW, JOHN GOUGH.
May 15th, 1812.

III.

On some Preparations of Gold lately employed medicinally: *by* A. S. DUPORTAL, *M. D. &c.*, *and* TH. PELLETIER, *Apothecary**.

AFTER having enjoyed some reputation as a medicine, gold had ceased to be administered to the patient, and taken an opposite direction. Lately, however, Dr Chrestien of Montpellier, a physician of great reputation and successful practice†, has revived its use. He has employed it in siphylitic and lymphatic affections, and chiefly in Clark's mode‡. The preparations he uses are metallic gold in a

The use of gold as a medicine lately revived.

* Abridged from Ann. de Chim. vol. LXXVIII, p. 38.

† The gentleman through whose means Dr. Godden Jones became acquainted with the virtues of d'Husson's *eau medicinale* in the gout. C.

‡ From a passage in the sequel I imagine Clark is put for Clare; and that it means by rubbing on the inside of the cheek, or on the gums C.

state

state of minute division, oxide of gold precipitated by potash, the oxide precipitated by tin, and the triple muriate of gold aud soda. These he considers as superior to mercurials. Some experiments by Mr. Vauquelin on the preparations of gold thus introduced into notice, have already been given*, and we shall now present our readers with some remarks on the subject by the gentlemen above mentioned, one of whom enjoyed the advantage of a personal acquaintance with Dr. Chrestien, at Montpellier.

Gold in powder. How prepared by Dr. Chrestien.

The first preparation of gold employed by this physician was the metal in a state of minute division. To obtain this, he formed an amalgam, by triturating leaf gold with seven times its weight of mercury in a marble mortar with a glass pestle, and then expelling the mercury by means of a powerful lens in the height of summer, or dissolving it out by pure nitric acid.

Another mode recommended.

The present writers recommend rather to precipitate a solution of muriate of gold by a solution of sulphate of iron at a minimum, filtering, and washing the precipitate with water acidulated by muriatic acid, in order to dissolve out the oxide of iron mingled with the precipitated gold. When the gold is thoroughly dried, it is in the state of a deep brown powder, though in the metallic state; all metals losing their brilliancy by being minutely divided.

Solution of gold

To prepare the oxide of gold precipitated by potash, they direct one part of nitric acid at 40° [sp. gr. 1·396] to be mixed with four of muriatic acid at 12° [1·089]; and cupelled gold to be heated with eight times its weight of this menstruum in a matrass with a long, narrow neck, till it boils gently. When no more gold will dissolve at this temperature, the solution is to be poured off, and evaporated to dryness in another matrass by a gentle fire. The residuum of this evaporation is to be dissolved in distilled water, and filtered.

precipitated by [illegible].

The filtered solution is to be treated with potash, to separate from it the oxide of gold: but in this there are great difficulties, and the whole cannot be thrown down, without part of it being reduced to the metallic state.

* Journal, vol. XXX, p. 248.

The

The cause of this is not known; but the authors ascribe it, 1, to the formation of a soluble triple muriate, which takes place when the potash is poured into the solution of muriate of gold: 2, to the excess of acid always present in this muriate: 3, to the more or less caustic state of the alkali employed: 4, to the greater or less quantity of this substance added to the muriate of gold.

When a solution of caustic potash is poured into a saturated solution of gold by muriatic acid, a yellow precipitate is formed*, which, when collected on a filter, does not amount to more than 40 grs of oxide from 72 grs of the metal in the solution. The remaining liquid is of a very deep colour, and contains a triple muriate of gold and potash. A fresh quantity of the caustic alkali will cause no farther precipitation, unless the liquid be kept several hours in a gentle heat: but in this case a new precipitate will fall down, extremely bulky, and of a deeper colour than the former, and apparently at a different degree of oxidation. Several weeks are necessary to complete the precipitation; and even at last a certain portion of gold will remain, which must be thrown down by a slip of tin, if we would lose nothing.

Oxide of gold first thrown down: a triple muriate remains in solution: from which more alkali and heat throw down gold apparently in a different state of oxidation.

If the solution of gold be very acid, there will be scarcely any perceptible precipitation: and this might be expected, as the alkali finds a sufficient quantity of free acid, to form muriate of potash enough for the production of the triple salt. Indeed no precipitation at all ought to take place, when the solution is extremely acid: but here experience does not entirely agree with therory, for a very small quantity of oxide of gold is always produced.

Superfluous acidity of the solution to be avoided.

The causticity of the potash is of great importance; for, if the neutral carbonate be employed, no change will take place without the assistance of heat. This, expelling a considerable portion of carbonic acid gas, will alter the colour of the solution from yellow to greenish. If it be then filtered, traces of the purple oxide of gold will be found; and it will effervesce with acids, having its fine golden colour restored. A few drops added to a glass of water will not colour it; but, if the water be acidulated,

Causticity of the potash important.

Action of the carbonate.

* It is necessary to employ heat.

the

Crystals produced.

the colour will instantly appear. The same solution yields by evaporation white, transparent, alkaline crystals, interspersed with black spots. These crystals dissolve in water without colouring it; and on filtering the solution it passes through transparent, leaving a little gold on the filter. The addition of any acid however causes its colour to reappear.

Their nature.

What is the chemical nature of the crystals obtained? Though this was not minutely ascertained for want of time, it appears certain, that they were composed of carbonic and muriatic acid, potash, and gold: but whether constituting a quadruple salt, a trisule, or two salts, one the triple muriate of gold and potash, the other subcarbonate of potash, the authors cannot say; nor could they form any judgment from the figure of the salt.

Carbonate of potash separates copper from gold.

It may not be amiss to observe, that, in an impure nitromuriatic solution of gold, saturated carbonate of potash will precipitate the copper, without throwing down the gold, if no heat be employed.

Too much alkali not to be employed.

As too large a quantity of alkali, added to a solution of muriate of gold, will cause a portion of the precipitated oxide to be redissolved; it is necessary, to add the alkali cautiously, boil the solution at every addition of alkali, and separate the precipitate by filtration, whenever a sensible quantity appears.

The oxide to be washed but lightly.

The precipitate must be washed but slightly, it being partly soluble in water, as Mr. Vauquelin remarked; and it must be dried in the shade and in a cool place, otherwise it will be a mixture of oxide and metallic gold.

Test of its purity.

It may be known whether the oxide be pure, by treating it with muriatic acid, which in this case will dissolve it completely; but, if it be mixed with metallic gold, part will remain undissolved.

Oxide precipitated by tin.

The oxide of gold precipitated by tin, which Dr. Chrestien also recommends, may be obtained either with metallic tin, or with its solution.

Precipitate with metallic tin.

For the first, slips of tin well cleaned are to be put into an aqueous solution of muriate of gold. These will soon be covered with a layer of pulverulent matter, of a colour more or less deep; which will be renewed several times, after being removed

removed. When this ceases to be reproduced, the liquor is to be filtered, and the precipitate washed in distilled water, dried in the shade, and powdered. This is the purple powder of Cassius.

If the oxide of gold be precipitated by a solution of tin, it is of importance, that the tin be in a fixed state of oxidation, otherwise the product will vary both in its nature and quantity. A uniform solution may always be obtained by dissolving slips of tin in muriatic acid at 12° [1·089], filtering, evaporating to the point of crystallization, dissolving the crystals in pure water, and filtering again. Part of this solution should immediately be mixed with the liquid muriate of gold; and the union of the two salts produces a precipitate, which should be increased by adding fresh quantities of the muriate of tin, as long as any thing is thrown down; after which the precipitate is to be washed, dried, and powdered. The quantity obtained appears to depend on the quantity of water added to the solutions of gold and tin. The more they are diluted, the more tin is thrown down. One drachm of gold, the solution of which was mixed with ten quarts of water, mixed with a very dilute solution of tin, yielded near five drachms and half of a very fine purple precipitate.

Precipitate with solution of tin.

Preparation of the solution.

It does not appear to be a matter of indifference which of these two precipitations is used. When metallic tin is employed, the precipitate is brown; and the gold, if not in the metallic state, is nearly approaching it. On the contrary, the precipitate produced by muriate of tin at a minimum of oxidation is of a deep purple colour; and, though it contains a little metallic gold, has much more of the oxides of gold and of tin; whence, it is obvious, the efficacy of the two preparations cannot be the same.

Difference between the two precipitates.

The muriate of gold is so greedy of moisture, that it soon deliquesces, whence it can be employed only in the liquid state; and, as its great causticity renders even this difficult, Dr. Chrestien thought of uniting it with the muriate of soda; thus producing a triple muriate, less deliquescent, and less caustic.

Muriate of gold very deliquescent.

For this purpose a solution of muriate of gold in distilled water, obtained as described above, is to be employed; and it is particularly important, that this salt has not an excess of acid.

Triple muriate of gold and soda.

acid. Into this solution is to be poured an aqueous solution of pure decrepitated muriate of soda, so as to combine an equal quantity of the dry salt with the gold dissolved. The two solutions being mixed, the fluid is to be evaporated by a gentle heat in a glass capsule, taking care to stir it well toward the end of the process. When the mass is sufficiently dry, it is to be powdered while hot in a glass or stone mortar; and the powder is to be kept from moisture, which it attracts in a slight degree.

Management of the fire important.

In this preparation the management of the fire is of great importance: for, if the desiccation of the salt be not carried far enough, it will contain too much acid; and, if it be urged too far, it will be in part decomposed, and mixed with a little gold.

Dr. Chrestien's mode of employing these preparations.

The enlightened physician, who extols the use of these preparations, employs them externally and internally; but recommends them to be mixed with other substances, lest their action should be too violent, if given alone. Thus for a long time he did not give the triple muriate of gold and soda otherwise than mixed with twice its weight of a powder composed of starch, charcoal, and the lake used by painters. As the alumine of the last however might take up a portion of the muriatic acid, and the charcoal might revive the gold, Dr. Chrestien changed this powder for that of liquorice root, orris root, &c.

Beside this he joined the compounds of gold with extracts of the attenuant plants; sugar with which he forms lozenges; sirups, in which he dissolves them, &c. He mixes them also with Galen's cerate, when he wishes to promote suppuration, and with lard, when he would employ them in frictions on the soles of the feet after the method of Cyrillo.

This faulty.

The writers of the present article do not approve the combination of the preparations of gold with these different substances, as all vegetable and animal substances, dissolved or not, revive gold from its acid solution. They recommend them to be given alone, or dissolved in distilled water: or at least, if they must be mixed, to mix them as short a time as possible before they are used.

Instances of the efficacy

In this way Dr. Duportal asserts that he has found good effects from them in siphylitic complaints. In a chancre corroding

corroding one of the corpora cavernosa he found them of real advantage: but the most striking instance of their efficacy was in a cancerous ulcer, that had destroyed the upper lip, attacked the soft parts of the nose and left cheek, destroyed the square bones [os carrés], and rendered the maxillary bone carious. Being called to a consultation with Dr. Payen on this very serious case, in which all the common methods had been tried in vain, Dr. Duportal hoped to oppose the progress of the disease by the use of Dr. Chrestien's medicine assisted by attenuant extracts. In consequence the patient was directed daily to rub into the gums the triple muriate of gold and soda; and to take oxide of gold precipitated by potash, with pills composed of the extracts of white henbane, hemlock, and sharp-pointed toadflax. The ulcer was daily washed with Sydenham's liquid laudanum, sprinkled over with powder of red bark and camphor, and dressed with a digestive in which oxide of gold was mixed. Under this treatment, which has been continued two months gradually increasing the dose of the substances, the ulcer has assumed a promising appearance; the carious points have disappeared; the suppuration furnishes laudable pus in moderate quantity; the patient daily improves in flesh and strength; and there is every reason to believe, that this evident melioration will continue. That it cannot be ascribed to the means employed in conjunction with the preparations of gold is evident, for they had been used previous to these without effect.

of the preparations of gold.

IV.

Experiments on the Existence of Water in Muriate of Ammonia formed by the Combination of Muriatic Acid and Ammoniacal Gasses. By Mr. JOHN MURRAY, *Lecturer on Chemistry, Edinburgh.*

To Mr. NICHOLSON.

SIR,

I Have been prevented by different circumstances from bestowing any attention until lately on the objections, which have

Mr. Murray's experiment

have been made to the experiment in my last communication proving the existence of water in muriatic acid gas. This I have little reason to regret, as the deficiency has been amply supplied by the candid communication from Dr. Bostock and Dr. Traill in the supplement to your last volume. From the care with which their experiment appears to have been conducted, it must be regarded as nearly decisive of the question at issue; and the result coinciding so exactly with that which I had stated to be obtained, while it is at variance with that affirmed by Messrs. Davies, I might probably spare myself the task of taking any notice of the observations of my opponent. As I have executed some experiments however, which occurred to me on this subject, a brief account of the results may not be unacceptable to your chemical readers.

confirmed by Drs Bostock and Trail.

Admissions of its opponents.

It has been admitted, that the experiment which I have brought forward, if accurate, is conclusive on the subject of this discussion. It has also been admitted, that when the experiment is performed in the manner I described, the result is that which I stated to be obtained—a sensible and even a considerable portion of water being produced, when the salt formed by the combination of muriatic acid and ammoniacal gasses is exposed to heat. But to obviate the conclusion from this it has been asserted, that the salt, while it is transferring from the vessel in which it is formed to that in which it is heated, absorbs water from the atmosphere, and that this is the source of the water it affords. This explanation has been given on the authority of Mr. Davy, who, it it stated, performed the experiment without obtaining water when this source of fallacy was avoided. And Mr. J. Davy, who it seems was disposed to doubt of the accuracy of my experiment before he knew of this mode of accounting for its result, states, that he was informed of it by his brother; who farther told him, that, if he heated the salt without exposure to the air, he would obtain no water. He accordingly made the experiment as it is described in your Journal (vol. XXXI, p. 314), and found no water to be produced; but when the experiment was made in the manner I had performed it, "water in no inconsiderable quantity was evolved:" and thus, it is added, "we have a demonstration, that the water

Attempt to obviate the conclusion from it.

librated

librated in Mr. Murray's experiment was not derived from the muriatic gas, but from the atmosphere."

It might have been expected, that the first step these gentlemen would have taken, when they assigned this as the source of the water obtained, would have been to prove its reality; and to show by experimental evidence, that the salt on which they operated has the power of attracting water from the atmosphere. No such evidence however is given; but the existence of this property is inferred from the result of an experiment, which may have arisen from causes altogether different. Admitting for a moment the accuracy of their experiment, the obtaining water when the salt is heated after exposure to the air, while none is obtained when it is heated without this exposure, is no proof, that the water in the former case has been absorbed from the atmosphere; for in making the experiment in these two modes, the sole difference is not the admission or exclusion of the air, nor is the sole operation of the air that of affording moisture; there are other circumstances of difference equally important, and which it is easy to perceive must influence the result.

That muriate of ammonia attracts water from the air should have been proved.

The experiment of Messrs. Davies not conclusive.

Thus the principal difficulty in the original experiment, so as to render it conclusive, arises from the volatility of the ammoniacal salt, and the inconsiderable interval of temperature between that point at which any water it may contain can be separated from it by heat, and that point at which the salt itself will pass into vapour. In consequence of this it must require a nice regulation of temperature to obtain the one effect without the other; and from this very circumstance, even had water not been obtained in the experiment as I first performed it, it could not have been affirmed, that it did not exist in the salt. Now this difficulty it is obvious is much greater, when heat is applied to a thin layer of salt encrusted over the whole internal surface of a retort, than when it is applied to the same quantity of salt collected in mass at the bottom of a retort; and it must indeed be nearly impracticable to apply the heat in the former case with such a precise adaptation to the relative volatilities of the water and the salt, as to expel the former without volatilizing the latter. If the heat therefore is kept sufficiently low not to volatilize the salt, and especially if care is taken to keep it

Principal difficulty in the original experiment.

This particularly exists in their mode of executing it;

 still

still lower than this, it is possible, that there may b no apparent production of water. If the salt too has any power of absorbing water, inferior even to what these gentlemen suppose, it is evident, that the portion of it in the upper part and curvature of the neck of the retort must absorb the small portion of water, that may be volatilized by a moderate heat applied to the salt at the bottom, or in the body of the retort; and according therefore to the assumption they themselves maintain, no water ought to appear in this mode of making the experiment, even though the salt may contain it. Farther, if any pressure is present in consequence of the arrangement by which the air is excluded, (and Mr. Davy's experiment is not sufficiently described, to enable us to determine whether this were the case or not,) this must retard or prevent the separation of the water. And lastly, when the air is excluded, that agency of it by which it promotes the transition of every substance into vapour by heat, lately so well illustrated by Gay-Lussac*, is prevented from operating; and the same result with regard to the expulsion of water in vapour from any matter containing it cannot be obtained, as when a communication with the atmosphere is preserved. It was to obviate some of these circumstances, that I performed the experiment in the manner in which it was originally executed. All of them however are neglected by Messrs. Davies, though it is obvious, that their influence must be important; and to account for the result they are said to have obtained, the *supposition* is introduced of the salt attracting water from the atmosphere, without any experimental evidence being given, that it has any such power.

and ought, on their own principle, to prevent the appearance of water.

Other objections to their experiment.

The cause they assume for the appearance of water fallacious.

I was satisfied prior to any experimental investigation, that the cause thus hypothetically assigned is altogether fallacious. When a soluble substance attracts water from the atmosphere, it continues to attract it, until it becomes humid, and is at length dissolved. This is the case with potash, muriate of lime, acetate of potash, and indeed every salt known to absorb water from the air; and it follows from the very property itself. The deliquescent substance imbibes water in consequence of the strong attraction it has to it; and this attraction must continue to operate, until an equilibrium between it

* Mémoires D'Arcueil, tom. 1, p. 204.

it and the force of cohesion is attained; and in a soluble substance therefore must continue until it is dissolved. No such property belongs however to muriate of ammonia; every chemist knows, that in an atmosphere in a common state of dryness it is not deliquescent, but remains dry for any length of time. There is no reason to believe, that it is capable of absorbing water short of that quantity, which shall produce sensible humidity; and it is altogether an extravagant assumption, that it can absorb water with such rapidity, as in a few minutes to imbibe that considerable quantity which it yields when exposed to heat. With regard to any hygrometric effect from the loose pulverulent state of the salt, it is not less extravagant to suppose, that it could operate so speedily, or to such an extent as is necessary to account for the result of the experiment; or that it could operate after the salt had been heated, so as to enable it to afford the quantity which even then it yields*.

The salt cannot act hygrometrically.

Fortunately the determination of this point is not attended with any peculiar difficulty. It may be ascertained by experiment, whether the salt does absorb water or not from the air, and whether the water which it yields when heated is derived from this source.

The point easily determined.

I first performed the experiment of heating the salt without its having been exposed to the air. In a small retort, over dry quicksilver, I combined in successive portions 25 cubic inches of ammoniacal gas, which had been dried by exposure to lime, with muriatic acid gas, which had been exposed to muriate of lime, adding at the end an excess of ammoniacal gas to fill the retort. The retort was then turned over in such a manner, that the extremity of its neck was kept under the quicksilver, and an inverted jar filled with quicksilver was placed over it. The body of the retort being surrounded with sand, heat was applied by an Argand's lamp

Experiment described.

* After the salt has afforded water by being heated, I had found it to afford an additional portion, when it is exposed to a stronger heat. It is also stated in your Journal (vol. XXXI, p. [illegible],) that water may be obtained from the salt successively by heating it repeatedly, if it is exposed to the atmosphere for a few minutes each time; and it is added, that in this way I might have obtained water to the amount of thrice the weight of the salt. No doubt, if the salt thus absorbs water, it may continue to afford it to fifty times its weight.

with

with a double wick; and afterward the heat was applied to the naked retort. In about ten minutes moisture appeared in the neck, and continued to accumulate, so that a dew covered a space of about two inches in length, and united into small globules. At the end of the experiment the salt was found to be sublimed entirely into the upper part of the body of the retort, and the curvature of its neck.

This repeated in different ways.

This experiment was repeated under different forms. In one, the two gasses were combined in small successive portions in the upper part of a long glass tube over dry quicksilver. The combination being completed, the tube, which had such a degree of curvature towards the middle of it, that, when placed horizontally, its extremity could be kept immersed in quicksilver, was turned over into this horizontal position, and ignited charcoal was placed around part of it, so that heat was communicated to its closed extremity, where the salt was collected, sufficient to volatilize it. Moisture in this case also was condensed on the sides of the tube. And in all the experiments, which were performed, sensible quantities of water were obtained*.

Exposure of the salt to the air for 15′ had no effect on the result.

I next repeated the experiment in another form. The salt was formed by the combination of the gasses in the retort, or in the tube as before. But previous to applying heat to it, it was left exposed for fifteen minutes to the air. The extremity of the neck of the retort or of the tube being then immersed in quicksilver, heat was applied as before, and as nearly as possible in the same manner, and to the same extent. The condensation of moisture was soon apparent, but the quantity was not greater, so far as could be estimated, than was obtained from the salt heated without having been exposed to the atmosphere. This exposure therefore, when the other circumstances of the experiment were the same, had no influence on the result.

Experiment to ascertain, whether the

I next proceeded to ascertain by more direct experiment, whether the salt does attract any moisture or not from the atmosphere. A glass bottle of the capacity of six cubic

* A few of the words toward the close of this paragraph were so obliterated by the seal, that it was necessary to supply them by conjecture. C.

inches

inches was filled with dry ammoniacal gas; muriatic acid gas, which had been exposed to muriate of lime, was added to it over dry quicksilver, and successive portions of the two gasses were introduced, until about 24 cubic inches of muriatic acid gas had been combined, the salt formed condensing over nearly the whole internal surface of the bottle. It was then filled with dry ammoniacal gas, and, a stopper fitted to it being introduced under the quicksilver, it was removed, and accurately weighed in a very sensible balance. The stopper was removed for a moment to allow the ammoniacal gas to escape, and atmospheric air to enter in its place. The bottle gained immediately 0·6 of a grain in weight from the substitution of the one air for the other. The stopper was again removed, and was placed in the scale, and no farther weight was gained. At the end of five minutes it remained perfectly the same, at the end of ten minutes it still remained exactly balanced; at fifteen minutes it was still stationary; at twenty minutes there appeared to be a very slight indication of increase of weight in the bottle; at the end of half an hour from the commencement of the weighing this was more apparent, and amounted to about 2° on the scale of the balance; at the end of an hour it had increased to 5°; and at the end of two hours to 10°. This total increase was found equal to 0·5 of a grain; the salt collected from the bottle weighed 13 grs; being wrapped loosely in paper it remained perfectly dry; and after two days its weight was found to be as nearly as possible the same.

salt attract moisiure,

The result of this experiment proves, that muriate of ammonia formed by the combination of muriatic acid and ammoniacal gasses absorbs no moisture from the air, or at least none which can account for the production of water from it when it is exposed to heat. Two or three minutes are sufficient to transfer it from the vessel in which it is formed to that in which it is heated. During this time, and even for 15 minutes, it does not absorb the smallest portion of moisture, for it gains no weight whatever; at the end of an hour the increase of weight was not more than 0·25 of a grain; and the total increase at the end of two hours was not equal to one fourth of the weight of water, which the salt yields by heat. Nor is there any certainty, that

proves that it does not.

that any part of this increase of weight arose from the absorption of humidity. It was more probably owing to the ammoniacal gas not being entirely expelled, when the stopper was first withdrawn, but being retained by a slight force in the interstices of the salt, and being only slowly detached by the atmospheric air. It was also not a uniform result, and in a subsequent experiment, in which the gasses were combined in a globe furnished with a stop cock, there was rather a very slight diminution of weight. The two experiments therefore are conclusive in proving, that this salt does not absorb humidity from the atmosphere.

Farther proof, that it does not.

One other experiment afforded a very satisfactory demonstration, that the muriate of ammonia formed by the combination of its constituent gasses has no power of absorbing water either by chemical attraction, or by what is named hygrometric affinity. In the experiment in which water was expelled from the salt by heat, the mouth of the retort or of the tube was closed, and the moisture condensed on its sides was thus submitted to the action of the salt in the most favourable manner; it must therefore have been quickly absorbed, had the salt any power of attracting water, such as has been supposed; but it remained without any diminution for a number of hours; and even after twenty-four hours the globules of water remained apparent. It is impossible to conceive a result, which can prove more satisfactorily that the salt has no such power.

These experiments conclusive.

These experiments then I consider as conclusive in refuting the supposition, by which it has been attempted to account for the water afforded by this salt when it is heated, that it is water which it has absorbed from the atmosphere. It is found, that it affords water when it is heated without having been exposed to the atmosphere; that the quantity it does afford is, as nearly as can be estimated, as great as that which it yields when it has been previously exposed; that it does not absorb humidity from atmospheric air in its usual state of dryness; and that it does not even reabsorb the water, which has been expelled from it by heat. The original experiment then, I trust, I may consider as remaining in full force, and as affording a conclusive proof of the existence of water in muriatic acid gas, and a proof of course of the falsity of the hypothesis which Mr. Davy has endeavoured to defend. I

Remarks on Mr. Davy's last communication.

I have no wish to enter on the discussion of the remaining observations of Mr. J. Davy in his last communication. I only feel myself called on to make one or two remarks on his assertion with regard to the accuracy of his own and his brother's experiments, and the inaccuracy of mine. He thinks proper to say, that all my experiments have been found to be incorrect; that I have advanced no arguments, that have not been answered, no experiments, the accuracy of which has been admitted. I shall merely meet these assertions by recalling in a very brief manner to the notice of your readers, the fact originally established by my experiments, the various kinds of denial which Messrs. Davies gave to them, and the admission which they have at length been compelled to yield to them.

The conversion of carbonic oxide into carbonic acid by oximuriatic gas first denied by Messrs. Davies

At the commencement of this controversy I had affirmed, that, when dry carbonic oxide, hidrogen, and oximuriatic acid gasses are submitted to mutual action, the carbonic oxide disappears, and carbonic acid is obtained. They opposed to this the supposition, that the conversion of carbonic oxide into carbonic acid was owing to the decomposition of water admitted to examine the product; or to the presence of atmospheric air, or the intermixture of a compound of oximuriatic acid and oxigen in the oximuriatic gas I employed: and they affirmed, that, when these sources of fallacy were avoided, and particularly when ammonia was employed to condense the product, the carbonic oxide remained unchanged, and no carbonic acid was formed*. Though satisfied of the futility of these suppositions, I repeated the experiment, with this variation; and still obtained the same result, the disappearance of the carbonic oxide, and the production of carbonic acid when the salt formed by the ammonia was decomposed by an acid. Still Messrs. Davies attempted to deny these results; and to support them in this denial they had recourse to some very singular methods†. They repeated my experiment to prove it incorrect, but instead of executing it in the manner in which I had performed it, as common candour, and common accuracy required, they diminished

* Journal, vol. XXVIII, p. 200, &c. vol. XXIX, p. 235.

† Ibid, vol. XXIX, p. 42, 188.

the

the proportion of hidrogen to less than one half, (using four measures to ten of carbonic oxide instead of equal measures) thus not only altering it in a material circumstance, but withdrawing as far as possible the very circumstance, which I had held essential to its success. And to prove, that the results of my experiments had arisen from the presence of atmospheric air, or of moisture in the gasses, they brought forward an experiment, in which both these were allowed to operate, instead of being excluded; and then contended, that the partial conversion of carbonic oxide into carbonic acid, which they did obtain, arose from the very sources of fallacy, which it ought to have been their care to exclude, but which they thus chose to admit.

then admitted by Mr. J. Davy,

At length, after all these attempts, Mr. J. Davy announced the discovery of a new gas, a compound as he supposed of oximuriatic acid and carbonic oxide, by the operation of which he farther supposed the formation of carbonic acid might be accounted for in conformity to his brother's hypothesis; and then he at once admitted what I had uniformly asserted, and what he and his brother had before as steadily denied, that the carbonic oxide disappears, and that carbonic acid is obtained, when the ammoniacal salt is decomposed by an acid. "Repeating my experiment on the exposure of the three gasses to light," he detected, "after the addition of ammonia, no traces of carbonic oxide;" and he perceived "an effervescence of the "ammoniacal salt with nitric acid," which effervescence he farther admits to be owing to carbonic acid*. These are the precise results I had obtained. How then can Mr. J. Davy venture to assert, that there are no experiments of mine the accuracy of which has been admitted? or how does he reconcile the admissions he now makes with the former positive assertions by himself and his brother, that, in the mutual action of these three gasses, the carbonic oxide remains unchanged, and no carbonic acid is formed?

but said to be effected indirectly.

There is one mode indeed, by which he throws some obscurity over this result of the controversy. He maintains, that the production of carbonic acid in these experiments is effected in an indirect mode; the oximuriatic acid

* Journal, vol. XXX, p. 30, vol. XXXI, p. 311.

and

and the carbonic oxide he supposes combine and form an acid gas, which unites with the ammonia; and when the salt formed by this union is decomposed by an acid, this gas he imagines decomposes water, and forms muriatic and carbonic acids. I have already given my reasons, which I need not repeat, for considering every thing relating to this gas as at present in the highest degree doubtful; and with regard to its supposed agency in decomposing water I also pointed out to him an inconsistency in his statement, which he calls imaginary, but which is real, and remains still unexplained. While he supposed, by a very circuitous mode of reasoning, that it decomposes water, I observed to him, that he had not ascertained the fact; and that he had even stated as one of the properties of this gas, that it is "very slowly absorbed by water," a statement directly at variance with the supposition, that it decomposes water; for the result of this decomposition must be an instantaneous reduction of volume by the absorption of the muriatic acid, which is one of its products, and a rapid absorption of the carbonic acid, which is its other product. He has accordingly since stated, that the gas, immediately on coming into contact with water, is decomposed, and converted into carbonic and muriatic acid gasses: and he adds "in my first notice of the gas I mentioned its being apparently slightly absorbed by water only among its most obvious qualities, those which made the first impression on me, and led me to consider it as a new substance." But he forgets to explain how in a result so obvious, and in which there appears to be no room for fallacy, he should first have found, that this gas is very slowly absorbed by water; and afterward, when I had pointed out to him that this was incompatible with his supposition that it decomposes water, that he should have discovered, that immediately on coming into contact with water it is resolved into muriatic and carbonic gasses, which must be quickly absorbed.

Inconsistency in his statement still unexplained.

These are points however, on the consideration of which it is not necessary to enter. Whatever importance may be attached to them as connected with the discussion on the nature of oximuriatic acid, they are of no importance in

Farther remarks on the subject.

regard

Farther remarks on the subject.

regard to the ultimate results of the experiments. The question in this point of view is not how carbonic acid is formed, whether directly or indirectly, but whether it is formed at all. Messrs. Davies affirmed, in contradiction to what I stated, that *it is not formed.* Mr. J. Davy now admits, that *it is formed*; and he may account as he is able for these opposite assertions: or, to remove the slight ambiguity which arises from involving the statement of the fact of the production of carbonic acid with the inquiry as to the manner in which it is produced, let the question be restricted to the effect on the carbonic oxide. I had uniformly affirmed, that it disappears. Messrs. Davies asserted, as the results of repeated experiments, that it remains unchanged*. But Mr. J. Davy now tells us, that it does disappear, so that no traces of it can be discovered after the addition of ammonia. On this I shall offer no comment, but rest satisfied with the simple statement of the fact; and if Mr. J. Davy after this thinks proper to repeat his assertions on the accuracy of his and his brother's experiments, and on the inaccuracy of mine, I shall certainly not feel it incumbent on me to take any notice of them. Allow me to add, that I regret having been compelled to make these observations; but I conceive I should be wanting in what I owe to myself, did I not repel assertions so injurious and unwarranted; and I believe I have done so in terms less severe than what the occasion might justify.

Sir H. Davy's opinion not a theory.

What farther relates to the general reasoning on this controversy, I leave altogether to the judgment of your readers. Mr. J. Davy "confesses himself totally at a loss to understand" how I have shown what he calls the theory of his brother (though strictly speaking it is entitled to neither of these appellations) to be an hypothesis: he still considers it he informs us as an expression of facts in all its essential parts, to the exclusion of hypothesis; and I have advanced it seems no arguments, that have not been answered.

I had supposed Mr. J. Davy to have been peculiarly unfortunate in his attempts to answer these arguments; and

* Journal, vol. XXVIII, p. 201, vol. XXIX, pp. 42, 235.

had

had supposed the question, whether this doctrine is a theory or an hypothesis, to have been brought into that point of view, that it was too obvious to bear any farther discussion. I may be mistaken in this; but still I cannot persuade myself, that there is any necessity for my entering on any recapitulation or extension of the arguments I have employed. With many of your readers they may have more weight than with my opponents; and my want of success in the latter respect, it is possible, may be owing not so much to deficiency in the argument, as in the person to whom it is addressed; for one who, like Mr. J. Davy, could not distinguish between an inference from a fact, and the expression of the fact itself*; who could confound an insulated fact, which his hypothesis did not explain, with an ultimate fact of which no explanation was to be expected, and who could call this fact one of the axioms of the science†; can hardly be expected, even with the most candid dispositions, to discriminate very accurately between the nicer limits, by which theory and hypothesis are defined. I shall not attempt therefore to convince this gentleman, but shall leave him in full possession of the belief (if he seriously entertains it) that he has answered all my arguments, refuted all my experiments, and established his brother's opinion as a genuine theory.

Relation of muriatic acid to water not peculiar.

I shall only add, that the late progress of chemical discovery has shown, that there is nothing peculiar in the relation of muriatic acid to water, such as is maintained in the common doctrine. The able researches of Gay-Lussac and Thenard and of Berthollet have shown, that all the more powerful acids, the sulphuric, nitric, phosphoric, and fluoric, contain combined water, from which they cannot be obtained free in an insulated state. Those of your readers, who feel an interest on this subject, will find a summary of these researches in the supplement to the second edition of my System of Chemistry, lately published.

I have the honour to be,

Your most obedient servant,

Edinburgh, May 31, 1812. JOHN MURRAY.

* Journal, vol. XXIX, pp. 39, 195.

† Ibid, vol. XXVIII, pp. 199, 302.

V.

V.

METEOROLOGICAL JOURNAL.

1812.	Wind	Pressure. Max.	Min.	Med.	Temperature. Max.	Min	Med.	Evap.	Rain	
5th Mo.										
May 4	N E	29·86	29·78	29·820	63	38	50·5	—		☽
5	E	30·01	29·86	29·935	64	40	52·0	—		
6	E	30·01	29·98	29·995	60	42	51·0	—		
7	E	29·94	29·86	29·900	58	45	51·5	·70		
8	S E	29·86	29·73	29·795	76	51	63·5	—		
9	S W	29·78	29·68	29·730	72	53	62·5	·90	—	
10	W	29·82	29·56	29·690	64	56	60·0	—	·20	
11	N W	29·56	29·54	29·550	65	49	57·0	—	·02	●
12	S W	29·53	29·51	29·520	65	44	54·5	—	·13	
13	S	29·56	29·50	29·530	60	40	50·0	·44	·15	
14	S	29·75	29·56	29·655	58	40	49·0	—	—	
15	N E	29·95	29·75	29·850	57	43	50·0	—	·10	
16	N	30·00	29·95	29·975	62	45	53·5	·36	2	
17	N E	29·95	29·87	29·910	53	45	49·0	—	7	
18	E	29·87	29·80	29·835	63	48	55·5	—		☾
19	E	29·80	29·63	29·715	66	53	59·5	—	·44	
20	S W	—	—	—	65	53	59·0	—	8	
21	Var.	29·94	29·63	29·785	61	45	53·0	—	·60	
22	N W	30·18	29·98	30·080	52	35	43·5	—		
23	E	30·27	29·98	30·125	61	40	51·5	·32		
24	S E	30·27	30·11	30·190	57	52	54·5	—	4	
25	S W	30·11	29·98	30·045	62	53	57·5	—		
26	S W	29·98	29·55	29·765	72	55	63·5	·43		○
27	S	29·59	29·55	29·570	71	51	61·0	—		
28	S E	29·69	29·59	29·640	69	54	61·5	—	14	
29	S W	29·84	29·69	29·765	72	53	62·5	—	·23	
30	S	29·76	29·74	29·750	67	52	59·5	·65		
31	S	29·75	29·72	29·735	65	54	59·5	—	·10	
6th Mo.										
June 1	S W	29·95	29·72	29·835	60	46	53·0	28	4	
		30·27	29·50	29.810	76	35	53·46	4·08	2·36	

The observations in each line of the Table apply to a period of twenty-four hours beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

NOTES.

NOTES.

Fifth Month. 4, 5, 6, Much dew. 7. Windy. 8. Windy: cirro-cumulus and cumulo-stratus: wind S. above: thunder clouds: the evening twilight was luminous and coloured: the clouds dispersing, and scattered in loose flocks over the rich ground of the western sky, presented a striking appearance. 9. Shower very early: Wind S. cirrus, cirro-cumulus: evening, much wind. 10. A. m. overcast: a gale from the W. with much cloud: showers: p. m. clear and pleasant; 11. A shower early: cumulo-stratus prevails. 12. Showers. 13. A thunder shower, with hail, about 3 p. m. 14. Showers. 15, 17. Cloudy, windy. 18. A. m. small rain: wind N. gentle: p. m. sunshine. 19. A. m. Wind E. pretty strong: clouds of different kinds, with haze above: p. m. thunder clouds: in the evening came on a violent thunder storm, which lasted several hours; it was chiefly to the S. and W. The appearances were very similar to those of the destructive hail storm, which occurred here in the same month, and on the same day of the month, and nearly at the same time of the day, in 1809: sheets of blue and white lightning came in quick succession, with an almost continual rolling of thunder. We had however no hail (being only on the flank of the storm) but sudden and heavy showers of warm rain; which was of the same amount in the upper as in the lower gauge. At 11, p. m. wind N. E. it still lightned far in the N. 20. A. m. wind W. cloudy and misty. 23. About noon, during a shower, it thundered to the southward. 29. A little thunder to the S. W. about 4 p. m. with a few drops: wet night. 31. An electric shower about 9, a. m. Nimbi: windy night.

RESULTS.

Winds variable.

Barometer: highest observation 30·27 inches; lowest 29·50 inches; Mean of the period 29·810 inches.

Thermometer: highest observation 76°; lowest 35°; Mean of the period 55·46°.

Evaporation 4·08 inches. Rain 2·26 inches.

PLAISTOW. L. HOWARD.
Sixth Month, 1812.

VI.

Account of the Pitch Lake of the Island of Trinidad. By NICHOLAS NUGENT, *M. D., Hon. Mem. of the Geol. Soc.**

Visit to the pitch lake of Trinidad.

BEING desirous to visit the celebrated Lake of Pitch, previously to my departure from the island of Trinidad, I embarked with that intention in the month of October, 1807, in a small vessel at Port Spain. After a pleasant sail of about thirty miles down the gulf of Paria, we arrived at the point la Braye, so called by the French from its characteristic feature. It is a considerable headland, about eighty feet above the level of the sea, and perhaps two miles long and two broad. We landed on the southern side of the point, at the plantation of Mr. Vessigny: as the boat drew near the shore, I was struck with the appearance of a rocky bluff or small promontory of a reddish brown colour, very different from the pitch which I had expected to find on the whole shore. Upon examining this spot, I found it composed of a substance coresponding to the porcelain jasper of mineralogists, generally of a red colour, where it had been exposed to the weather, but of light slate blue in the interior: it is a very hard stone with a conchoidal fracture, some degree of lustre, and is perfectly opake, even at the edges: in some places, from the action of the air, it was of a reddish or yellowish brown, and an earthy appearance. I wished to have devoted more time to the investigation of what in the language of the Wernerian school is termed the geognostic relations of this spot, but my companions were anxious to proceed. We ascended the hill, which was entirely composed of this rock, to the plantation, where we procured a negro guide, who conducted us through a wood about three quarters of a mile. We now perceived a strong sulphureous and pitchy smell, like that of burning coal, and soon after had a view of the lake, which at first sight appeared to be an expanse of still water, frequently interrupted by clumps of dwarf trees, or islets of rushes and shrubs: but

Porcelain jasper.

The lake described.

* Trans. of the Geol. Society, vol. I, p. 69.

on

on a nearer approach we found it to be in reality an extensive plain of mineral pitch, with frequent crevices and chasms filled with water. The singularity of the scene was altogether so great, that it was some time before I could recover from my surprise so as to investigate it minutely. The surface of the lake is of the colour of ashes, and at this season was not polished or smooth so as to be slippery; the hardness or consistence was such as to bear any weight, and it was not adhesive, though it partially received the impression of the foot; it bore us without any tremulous motion whatever, and several head of cattle were browsing on it in perfect security. In the dry season however the surface is much more yielding, and must be in a state approaching to fluidity, as is shown by pieces of recent wood and other substances being enveloped in it. Even large branches of trees, which were a foot above the level, had in some way become enveloped in the bituminous matter. The interstices or chasms are very numerous, ramifying and joining in every direction, and in the wet season being filled with water, present the only obstacle to walking over the surface; these cavities are generally deep in proportion to their width, some being only a few inches in depth, others several feet, and many almost unfathomable: the water in them is good and uncontaminated by the pitch; the people of the neighbourhood derive their supply from this source, and refresh themselves by bathing in it: fish are caught in it, and particularly a very good species of mullet. The arrangement of the chasms is very singular, the sides, which of course are formed of the pitch, are invariably shelving from the surface, so as nearly to meet at the bottom, but then they bulge out towards each other with a considerable degree of convexity. This may be supposed to arise from the tendency in the pitch slowly to coalesce, whenever softened by the intensity of the Sun's rays. These crevices are known occasionally to close up entirely, and we saw many marks or seams from this cause. How these crevices originate it may not be so easy to explain. One of our party suggested, that the whole mass of pitch might be supported by the water, which made its way through accidental rents, but in the solid state it is of greater specific gravity than water, for

Branches of trees above its level enveloped with pitch.

Chasms in the pitch

filled with good water,

containing fish.

Islets in the lake.

several bits thrown into the pools immediately sunk*. The lake (I call it so because I think the common name appropriate enough) contains many islets covered with long grass and shrubs, which are the haunts of birds of most exquisite plumage, as the pools are of snipe and plover. Alligators are also said to abound here, but it was not our lot to encounter any of these animals. It is not easy to state precisely the extent of this great collection of pitch; the line between it and the neighbouring soil is not always well defined, and indeed it appears to form the substratum of the surrounding tract of land. We may say, however, that it is bounded on the north and west sides by the sea, on the south by the rocky eminence of porcelain jasper, before mentioned, and on the east by the usual argillaceous soil of the country; the main body may perhaps be estimated at three miles in circumference; the depth cannot be ascertained, and no subjacent rock or soil can be discovered. Where the bitumen is slightly covered by soil, there are plantations of cassava, plantains, and pine apples, the last of which grow with luxuriance, and attain to great perfection. There are three or four French and one English sugar estates in the immediate neighbourhood; our opinion of the soil did not, however, coincide with that of Mr. Anderson, who, in the account he gave some years ago, thought it very fertile. It is worthy of remark, that the main body of the pitch, which may properly be called the lake, is situate higher than the adjoining land, and that you descend by a gentle slope to the sea, where the pitch is much contaminated by the sand of the beach. During the dry season, as I have before remarked, this pitch is much softened, so that different bodies have been known slowly to sink in it; if a quantity be cut out, the cavity left will be shortly filled up: and I have heard it related, that when the Spaniards

Its extent.

Vegetation on it where there is a thin soil.

The surface of the pitch higher than that of the neighbouring land.

Much softened in the dry season.

* Pieces of asphaltum are, I believe, frequently found floating on the Dead Sea in Palestine, but this arises probably from the extraordinary specific gravity of the waters of that lake, which Dr. Marcet found to be 1·211. Mr. Hatchett states the specific gravity of ordinary asphaltum to vary from 1·023 to 1·165, but in two varieties of that of Trinidad it was as great as 1·336 and 1·744, which led Mr. Hatchett to form a conjecture, which I shall afterwards notice.

niards undertook formerly to prepare the pitch for economical purposes, and had imprudently erected their cauldrons on the very lake, they completely sunk in the course of a night, so as to defeat their intentions. Numberless proofs are given of its being at times in this softened state: the negro houses of the vicinage, for instanec, built by driving posts in the earth, frequently are twisted or sunk on one side. In many places it seems to have actually overflown like lava, and presents the wrinkled appearance which a sluggish substance would exhibit in motion.

The substance varies much.

This substance is generally thought to be the asphaltum of naturalists: in different spots however it presents different appearances. In some parts it is black, with a splintery conchoidal fracture, of considerable specific gravity, with little or no lustre, resembling particular kinds of coal, and so hard as to require a severe blow of the hammer to detach or break it; in other parts, it is so much softer, as to allow one to cut out a piece in any form with a spade or hatchet, and in the interior is vesicular and oily; this is the character of by far the greater portion of the whole mass; in one place, it bubbles up in a perfect fluid state, so that you may take it up in a cup, and I am informed, that in one of the neighbouring plantations there is a spot where it is of a bright colour, shining, transparent, and brittle, like bottle glass or resin. The odour in all these instances is strong and like that of a combination of pitch and sulphur. No sulphur however is any where to be perceived, but from the strong exhalation of that substance and the affinity which is known to exist between the fluid bitumens and it, much is, no doubt, contained in a state of combination; a bit of the pitch held in the candle melts like sealing wax, and burns with a light flame, which is extinguished whenever it is removed, and on cooling the bitumen hardens again. From this property it is sufficiently evident, that this substance may be converted to many useful purposes, and accordingly it is universally used in the country wherever pitch is required; and the reports of the naval officers who have tried it are favourable to its more general adoption; it is requisite merely to prepare it with a proportion of oil, tallow, or common tar, to give it a sufficient degree of fluid-

It smells of sulphur;

melts in the flame of a candle, and hardens on cooling.

Used as pitch.

 ity.

ity. In this point of view, this lake is of vast national importance, and more especially to a great maritime power. It is indeed singular, that the attention of government should not have been more forcibly directed to a subject of such magnitude: the attempts that have hitherto been made to render it extensively useful have for the most part been only feeble and injudicious, and have consequently proved abortive. This vast collection of bitumen might in all probability afford an inexhaustible supply of an essential article of naval stores, and being situate on the margin of the sea could be wrought and shipped with little inconvenience or expense*. It would however be great injustice to Sir Alexander Cochrane not to state explicitly, that he has at various times, during his long and active command on the Leeward Island station, taken considerable pains to insure a proper and fair trial of this mineral production for the highly important uses of which it is generally believed to be capable. But whether it has arisen from certain perverse occurrences, or from the prejudice of the mechanical superintendants of the colonial dock yards, or really, as some have pretended, from an absolute unfitness of the substance in question, the views of the gallant admiral have been invariably thwarted, or his exertions rendered altogether fruitless.

Its importance in this view.

I was at Antigua in 1809, when a transport arrived laden with this pitch for the use of the dock yard at English Harbour: it had evidently been hastily collected with little care or zeal from the beach, and was of course much contaminated with sand and other foreign substances. The best way would probably be to have it properly prepared on the spot, and brought to the state in which it may be serviceable, previously to its exportation. I have frequently seen it used to pay the bottoms of small vessels, for which it is particularly well adapted, as it preserves them from the numerous tribe of worms so abundant in tropical countries†.

Not fairly tried.

A preservative against worms.

* This island contains also a great quantity of valuable timber, and several plants which yield excellent hemp.

† The different kinds of bitumen have always been found particularly obnoxious to the class of insects; there can be little doubt but that they formed ingredients in the Egyptian compound for embalming bodies, and the Arabians are said to avail themselves of them in preserving the trappings of their horses. Vide Jameson's Mineralogy.

There

There seems indeed no reason why it should not, when duly prepared and attenuated, be applicable to all the purposes of the petroleum of Zante, a well known article of commerce in the Adriatic, or that of the district in Burmah, where 400000 hogsheads are said to be collected annually*.

Bitumen thrown up in the neighbouring sea.

It is observed by captain Mallet, in his Short Topographical Sketch of the Island, that "near Cape la Brea (la "Braye) a little to the south-west, is a gulf or vortex, "which in stormy weather gushes out, raising the water "five or six feet, and covers the surface for a considerable "distance with petroleum or tar;" and he adds, that "on "the east coast, in the Bay of Mayaro, there is another "gulf or vortex, similar to the former, which in the "months of March and June produces a detonation like "thunder, having some flame with a thick black smoke, "which vanishes away immediately; in about twenty-four "hours afterward is found along the shore of the bay a "quantity of bitumen or pitch, about three or four inches "thick, which is employed with success". Captain Mallet likewise quotes Gumilla, as stating in his Description of the Orinoco, that about seventy years ago, "a spot of land on "the western coast of this island, near half way between "the capital and Indian village, sunk suddenly, and was "immediately replaced by a small lake of pitch, to the "great terrour of the inhabitants".

Land swallowed up in a pitch lake.

Probably a subterraneous reservoir of it.

I have had no opportunity of ascertaining personally whether these statements are accurate, though sufficiently probable from what is known to occur in other parts of the world; but I have been informed by several persons, that the sea in the neighbourhood of la Braye is occasionally covered with a fluid bitumen, and in the south-eastern part of the island there is certainly a similar collection of this bitumen, though of less extent, and many such detached spots of it are to be met with in the woods: it is even said, that an evident line of communication may thus be traced between the two great receptacles. There is every probability, that in all these cases the pitch was originally fluid,

* Vide Aikin's Dictionary of Chemistry, quoted from Captain Cox in the Asiatic Researches.

an

and has since become inspissated by exposure to the air, as happens in the Dead Sea and other parts of the east.

Geological inquiries difficult in this country.

It is for geologists to explain the origin of this singular phenomenon, and each sect will doubtless give a solution of the difficulty according to its peculiar tenets. To frame any very satisfactory hypothesis on the subject, would require a more exact investigation of the neighbouring country, and particularly to the southward and eastward, which I had not an opportunity of visiting. And it must be remembered, that geological inquiries are not conducted here with that facility which they are in some other parts of the world; the soil is almost universally covered with the thickest and most luxuriant vegetation, and the stranger is soon exhausted and overcome by the scorching rays of a vertical sun. Immediately to the southward, the face of the country, as seen from la Braye, is a good deal broken and rugged, which Mr. Anderson attributes to some convulsion of nature from subterranean fires, in which idea he is confirmed by having found in the neighbouring woods several hot springs. He is indeed of opinion, that this tract has experienced the effects of the volcanic power, which, as he supposes, elevated the great mountains on the main and northern side of the island*. The production of all bituminous substances has certainly with plausibility been attributed to the action of subterranean fires on beds of coal, being separated in a similar manner as when effected by artificial heat, and thus they may be traced through the various transformations of vegetable matter. I was accordingly particular in my inquiries with regard to the existence of beds of coal, but could not learn that there was any certain trace of this substance in the island; and though it may exist at a great depth, I saw no strata that indicate it. A friend indeed gave me specimens of a kind of bituminous shale mixed with sand, which he brought from Point Cedar, about twenty miles distant; and I find Mr. Anderson speaks of the soil near the Pitch-lake containing burnt cinders, but I imagine he may have taken for them the small fragments of the bitumen itself.

Hot springs in the neighbouring woods.

No coal known to exist here.

An examination of this tract of country could not fail, I

* Vide 79th vol. Philos. Trans.; or Ann. Register for 1789.

think,

think, to be highly gratifying to those who embrace the Huttonian theory of the Earth, for they might behold the numerous branches of one of the largest rivers of the world (the Orinoco) bringing down so amazing a quantity of earthy particles as to discolour the sea in a most remarkable manner for many leagues distant*; they might see these earthy particles deposited by the influence of powerful currents on the shores of the gulf of Paria, and particularly on the western side of the island of Trinidad; they might there find vast collections of bituminous substances, beds of porcelain jasper, and such other bodies, as may readily be supposed to arise from the modified action of heat on such vegetable and earthy materials as the waters are known actually to deposit. They would further perceive no very vague traces of subterranean fire, by which these changes may have been effected, and the whole tract elevated above the ordinary level of the general loose soil of the country, as for instance, hot springs, the vortices above mentioned, the frequent occurrence of earthquakes, and two semivolcanic mounds at Point Icaque, which, though not very near, throw light on the general character of the country. Without pledging myself to any par-

Huttonian theory.

* No scene can be more magnificent than that presented on a near approach to the north-western coast of Trinidad. The sea is not only changed from a light green to a deep brown colour, but has in an extraordinary degree, that rippling, confused, and whirling motion, which arises from the violence of contending currents, and which prevail here in so remarkable a manner, particularly at those seasons when the Orinoco is swollen by periodical rains, that vessels are not unfrequently several days or weeks in stemming them, or perhaps are irresistibly borne before them far out of their destined track. The dark verdure of lofty mountains, covered with impenetrable woods to the very summits, whence, in the most humid of climates, torrents impetuously rush through deep ravines to the sea; three narrow passages into the gulf of Paria, between rugged mountains of brown micaceous schist, on the cavernous sides of which the eddying surge dashes with fury, and where a vessel must necessarily be for some time embayed, with a depth of water scarcely to be fathomed by the lead, present altogether a scene which may well be conceived to have impressed the mind of the navigator who first beheld it with considerable surprise and awe. Columbus made this land in his third voyage, and gave it the name of the *Bocas del Drago*. From the wonderful discoloration and turbidity of the water, he sagaciously concluded, that a very large river was near, and consequently a great continent.

Magnificent scene.

Inferences of Columbus.

ticular

ticular system of geology, I confess an explanation similar to this appears to me sufficiently probable, and consonant with the known phenomena of nature. A vast river, like the Orinoco, must for ages have rolled down great quantities of woody and vegetable bodies, which from certain causes, as the influence of currents and eddies, may have been arrested and accumulated in particular places; they may there have undergone those transformations and chemical changes, which various vegetable substances similarly situate have been proved to suffer in other parts of the world. An accidental fire, such as is known frequently to occur in the bowels of the Earth, may then have operated in separating and driving off the newly formed bitumen more or less combined with siliceous and argillaceous earths, which forcing its way through the surface, and afterward becoming inspissated by exposure to the air, may have occasioned such scenes as I have ventured to describe. The only other country accurately resembling this part of Trinidad of which I recollect to have read, is that which borders on the gulf of Taman in Crim Tartary: from the representation of travellers, springs of naphtha and petroleum equally abound, and they describe volcanic mounds precisely similar to those of Point Icaque. Pallas's explanation of their origin seems to me very satisfactory, and I think it not improbable, that the River Don and Sea of Azof may have acted the same part in producing these appearances in the one case, as the Orinoco and gulf of Paria appear to have done in the other*. It may be supposed that the destruction of a forest, or perhaps even a great Savanna on the spot, would be a more obvious mode of accounting for this singular phenomenon; but, as I shall immediately state, all this part of the island is of recent alluvial formation, and the land all along this coast is daily receiving a considerable accession from the surrounding water. The Pitch-lake with the circumjacent tract, being now on the margin of the sea, must in like manner have had an origin of no very distant date; besides, according to the above representation of Capt. Mallet, and which has been frequently corroborated, a fluid bitumen oozes up and rises to the sur-

Similar country in Tatary.

* Vide Universal Mag. for Feb. 1808, Mrs. Guthrie's Tour in the Tauride, or Voyages de Pallas.

face

face of the water on both sides of the island, not where the sea has encroached on and overwhelmed the ready-formed land, but where it is obviously in a very rapid manner depositing and forming a new soil.

From a consideration of the great hardness, the specific gravity, and the general external characters of the specimens submitted a few years ago to the examination of Mr. Hatchett, that gentleman was led to suppose, that a considerable part of the aggregate mass at Trinidad was not pure mineral pitch or asphaltum, but rather a porous stone of the argillaceous genus much impregnated with bitumen. Two specimens of the more compact and earthy sort, analysed by Mr. Hatchett, yielded about 32 and 36 per cent of pure bitumen: the residuum in the crucible consisted of a spongy, friable, and ochraceous stone; and 100 parts of it afforded, as far as could be determined by a single trial, of silica 60, alumine 10, oxide of iron 10, carbonaceous matter by estimation 11; not the smallest traces of lime could be discovered, so that the substance has no similarity to the bituminous limestones which have been noticed in different parts of the world*. I have already remarked, that this mineral production differs considerably in different places. The specimens examined by Mr. Hatchett by no means correspond in character with the great mass of the lake, which, in most cases, would doubtless be found to be infinitely more free from combination with earthy substances; though from the mode of origin which I have assigned to it, this intermixture may be regarded as more or less unavoidable. The analysis of the stone after the separation of the bitumen, as Mr. Hatchett very correctly observes, accords with the prevalent soil of the country; and I may add, with the soil daily deposited by the gulf, and with the composition of the porcelain jasper, in immediate contact with the bituminous mass.

Mr. Hatchett's supposition.

Specimens analysed by him.

All the country which I have visited in Trinidad, is either decidedly primitive or alluvial. The great northern range of mountains which runs from east to west, and is connected with the highlands of Paria on the continent by the

Geology of the island.

* Vide Linnean Trans. vol. 8.

islands

islands at the Bocas, consists of gneiss, of mica slate containing great masses of quartz, and in many places approaching so much to the nature of talc, as to render the soil quite unctuous by its decomposition, and of compact bluish gray limestone, with frequent veins of white crystallized carbonate of lime. From the foot of these mountains for many leagues to the southward there is little else than a thick, fertile, argillaceous soil, without a stone or a single pebble. This tract of land. which is low and perfectly level, is evidently formed by the *detritus* of the mountains, and by the copious tribute of the waters of the Orinoco, which, being deposited by the influence of currents, gradually accumulates, and in a climate where vegetation is astonishingly rapid, is speedily covered with the mangrove and other woods. It is accordingly observed, that the leeward side of the island constantly encroaches on the gulf, and marine shells are frequently found on the land at a considerable distance from the sea. This is the character of Naparima and the greater part of the country I saw along the coast to la Braye. It is not only in forming and extending the coast of Trinidad, that the Orinoco exerts its powerful agency; cooperating with its mighty sister flood, the Amazons, it has manifestly formed all that line of coast and vast extent of country, included between the extreme branches of each river. To use the language of a writer in the Philosophical Transactions of Edinburgh,

Land formed by the Oronoko and the river of Amazons.

" If you cast your eye upon the map you will observe, " from Cayenne to the bottom of the gulf of Paria, this " immense tract of swamp formed by the sediment of these " rivers, and a similar tract of shallow muddy coast, which " their continued operation will one day elevate. The " sediment of the Amazons is carried down thus to leeward " (the westward) by the constant currents which set along " from the southward and the coast of Brazil. That of the " Orinoco is detained and allowed to settle near its " mouths by the opposite island of Trinidad, and still more " by the mountains on the main, which are only separated " from that island by the Bocás del Drago. The coast of " Guinea has remained, as it were, the great eddy or resting " place for the washings of great part of South America

" for

"for ages; and its own comparatively small streams have "but modified here and there the grand deposit*."

Having been amply gratified with our visit to this singular place, which to the usual magnificence of the West Indian landscape unites the striking peculiarity of the local scene, we reembarked in our vessel, and stood along the coast on our return. On the way we landed, and visited the plantations of several gentlemen, who received us with hospitality, and made us more fully acquainted with the state of this island: a colony which may with truth be described as fortunate in its situation, fertile in its soil, and rich beyond measure in the productions of nature; presenting, in short, by a rare combination, all which can gratify the curiosity of the naturalist, or the cupidity of the planter; restrained in the developement of its astonishing resources, only by the inadequacy of population, the tedious and ill-defined forms of Spanish justice, and the severe, though we hope transient, pressure of the times.

Political view of the island of Trnidad.

VII.

Chemical Experiments on Indigo: by M. CHEVREUL†.

MR. Vauquelin having requested me to examine the cause of that purple smoke, which arises from indigo exposed to heat, I made some experiments for the purpose, of which the following are the results.

Examination of the purple smoke from indigo.

Sect. I. On distilling indigo with a gentle heat, the products were: 1, an ammoniacal water: 2, sulphur, united probably with oily hidrogen: 3, a thick oil of a brown colour, containing carbonate and acetate of ammonia: 4, prussiate and hidroguretted sulphuret of ammonia: 5, a purple

Action of heat on indigo.

* Vide Mr. Lochead's Obser. on the Nat. His. of Guiana, Edin. Trans. vol. 4. See Journal, 4to series, vol. II, p. 352.

† Journ. de Phys. vol. LXV, p. 309. Abridged from the paper read to the Institute, july the 13th, 1807. A fuller account is inserted in the Ann. de Chim. vol. LXVI, p. 5, of which the translator has occasionally availed himself.

purple matter crystallized in small silky tufts at the summit of the retort: 6, a very bulky nitrogenous coal, yielding a prussiate when calcined with potash: 7, some gasses, which I did not examine.

Best mode of obtaining the purple matter.

The purple matter being the principal object of my research, it was necessary to have recourse to some other mode of obtaining it in a state of purity, for that I obtained by distillation was contaminated with the oil, which arose with it. The process that succeeded best with me was heating in a platina or silver crucible, surrounded by a charcoal fire, 5 dec. [7·7 grs] of indigo in fine powder; when the purple matter crystallized in needles in the middle of the crucible. It is necessary that the crucible be kept well closed during the process, and also for some time after it is removed from the furnace, otherwise the indigo would take fire.

I shall describe below the properties of this sublimed matter, which had not wholly escaped the observation of Bergman; merely observing here, that it is the indigo separated from all those matters with which it is combined in what is sold by this name. At present I shall proceed to examine the nature of these substances, and the methods of separating them.

Analysis of indigo in the humid way. Action of water on it.

Sect. II. *Art.* I. Indigo finely powdered was infused for twelve hours in water heated to 90° or 100° F., in a closed glass vessel. The decanted liquor retaining some indigo in suspension, it was filtered; and the indigo was exhausted by repeated infusion and decoction.

Disoxigenated indigo.

a. These liquors being united and distilled yielded an odoriferous water, a little ammoniacal; and I suspect it contained also sulphur. Mean time a greenish powder was precipitated from it, which assumed a blue colour from contact with the air. This substance exhibited all the characters of indigo, whence I infer, that part of the indigo in that of the shops is disoxigenated, and dissolves in water by means of the ammonia.

Green matter.

b. Long after the separation of the disoxigenated indigo, a flocculent precipitate appeared of a peculiar substance, which I shall call *green matter*; and which had the following properties. It is very little soluble in water, unless by the intermedium of an alkali. It then assumes a reddish colour,

colour, which acids change to a green by saturating the alkali. When the solutions are concentrated, the green matter falls down in green flocks. Alcohol dissolves this precipitate, and forms a red tincture; but this, when spread out thin, or mixed with water, appears green, as it does when viewed on its surface.

c. Alcohol being added to the concentrated liquor *b*, from which the green matter had been precipitated, separated a substance, the taste of which was slightly bitter and astringent, and which burnt on the coals, diffusing a smell of empyreumatic vinegar. The alcohol acquired a reddish colour, owing to the combination of green matter with ammonia.

Thus the substances separated from the indigo by water were, 1, ammonia: 2, indigo at a minimum of oxidation: 3, a green matter: 4, a slightly bitter and astringent matter, of a yellowish brown colour. Of these the 2d and 3d are held in solution by the ammonia.

100 parts of indigo lost 12 by treatment with water.

Action of alcohol.

Art. II. From the indigo exhausted by water alcohol took up, 1, some green matter: 2, a matter that I call red resin: 3, indigo at a maximum of oxidation.

The insolubility of the green matter in the treatment with water (*Art.* I.) I ascribe to the want of a sufficient quantity of ammonia to dissolve it entirely, and the affinity of the red matter for it.

The principal difference between the red resin and the green matter is, that the latter is rendered red by alkalis, and that this compound becomes green by the addition of an acid; while the colour of the former is not changed either by acids or alkalis, only acids produce with it a red flocculent precipitate.

In acting twice on the indigo alcohol took up 26 parts from the 88 left by the water. I suffered the alcohol to act on it no longer, when it began to acquire a violet tint.

Action of muriatic acid.

Art. III. Muriatic acid dissolved 10 parts; 2 of which were iron mixed with alumine, 2 carbonate of lime, and 6 probably red matter, that was dissolved in the acid after being decomposed.

Farther action of alcohol.

The preceding experiment having shown, that the indigo was

was not completely divested of foreign colouring matter, I treated it again with alcohol, till this liquid became blue. By this treatment it lost 4 parts of red resin, mixed with a little indigo.

In these different processes the indigo lost 0·52 of foreign matter, which reduced it to 0·48, from which 0·03 more must be deducted for the silex it still contains.

Indigoes differ.

Every sort of indigo does not yield the same results on analysing as that of Guatimala, on which I operated. In most the green matter changed to a fawn colour; it became very red on the addition of alkalis; but acids did not render this compound green. One specimen, in pretty thick square cakes, of a black blue colour, yielded me no indigo at a minimum. Its ashes contained more iron than that of Guatimala, and also magnesia. Some indigo, which I was informed came from Bengal, yielded me a twentieth of indigo at a minimum; and its ashes contained a little sulphate of lime. In some indigoes I found traces of phosphate of lime.

Green matter variable.

It is not very common to find the green matter in full possession of its properties: sometimes yellow extractive matter is so predominant, that it is difficult to detect it; and sometimes no vestige of it is to be found. In general I remarked, that those indigoes, which contained most ammonia, contained also more indigo at a minimum, and more green matter, than others. The indigo of Java afforded me the last in its greatest purity.

Colouring matter variously modified.

I consider the colouring matters accompanying indigo as originating from the same substance variously modified.

Purple smoke

Sect. III. The source of the purple smoke was now easily detected. On heating successively the green matter, extract, and gum, extracted by water, and the red resin extracted by alcohol, no purple smoke was perceivable. But trying the same experiment on the indigo separated by water, on that separated by alcohol, and lastly on that treated successively by water, alcohol, and muriatic acid, a fine purple smoke arose, much deeper coloured than that produced by an equal quantity of indigo not purified.

the pure indigo sublimed.

This smoke is not the result of a decomposition of the indigo by heat: for we found by experiment, that it was this

this colouring matter itself volatilized; and that the substance crystallized in silky tufts, obtained by distilling indigo, is indigo in a state of purity. These crystals dissolve in concentrated sulphuric acid, imparting to it a fine blue colour; and are volatilized anew in a purple smoke, when thrown on a hot body.

Indigo, therefore, is volatile, and capable of crystallization; and may be purified either in the dry or in the wet way. The indigo obtained in both ways is perfectly similar, except that the latter always retains some earthy matter: and it is remarkable, that the indigo purified in the wet way is not so blue as it was before, and has a perceptible violet tinge; while indigo not purified, if placed by its side, appears of a dull blue. Purified indigo.

When pure indigo is thrown into concentrated sulphuric acid, it first produces a yellow, which afterward becomes green, and at length of a fine blue. In this process the indigo undergoes some change of composition, that merits examination. This is shown, by its being soluble in a number of menstruums, after it has been precipitated from this solution, which before had no action on it: and, which is more strange, by its no longer producing the purple smoke, at least in the same circumstances, and appearing to have lost its volatility. Action of sulphuric acid on it.

Hot alcohol dissolves a small portion of indigo, which gives it a fine blue colour; but as it cools the colouring matter falls down, and after some time scarcely any is retained in solution. If however the indigo contain a certain quantity of the red resin, the solution will remain coloured for some months. Action of alcohol.

From the facts adduced it follows:

1, That pure indigo is purple: General properties of indigo.

2, That it is volatilized in the form of a purple smoke, crystallizable in needles of the same colour:

3, This volatization of a highly carbonated substance is remarkable, as it demonstrates, that the volatility of compounds does not depend simply on the volatility of their elements, but also on the affinity, with which the most dilatable are united to the most fixed:

4, Indigo is a little soluble in alcohol.

A very

Indigo disoxidated by sulphuretted hidrogen.

A very interesting observation, for which we are indebted to Mr. Vauquelin, is the disoxidation of indigo by sulphuretted hidrogen. This experiment proves two curious facts: 1st, that in this substance either the whole or at least part of the oxigen exists in some sort separate from the other principles, since it may be taken away, and restored at pleasure by allowing the sulphuretted hidrogen to evaporate in the open air, without destroying the nature of the colouring matter. In this circumstance indigo has a resemblance to the metals. 2dly, that carbon has no concern in the colouring of indigo, since this is deprived of colour in circumstances in which it contains most carbon.

VIII.

On the Action of Muriatic Acid on Sugar, and the Nature of its Principles: In a Letter from JOHN NOWELL, *Esq.*

To W. NICHOLSON, Esq.

SIR,

Nitric acid decomposed on sugar by yielding up one of its elements, the oxigen, might furnish a clew for investigating its composition.

IT is well known, that the nitric acid becomes decomposed with sugar under certain circumstances, and forms a vegetable acid (the oxalic) by yielding to the sugar one of its elements. If the composition of the nitric acid was not known, this property evidently would furnish a clew to guide us in the investigation of its elementary principles. Some time ago I was struck with the same idea with respect to the muriatic acid; and, as its action on sugar had not been observed with attention, I set about making experiments on the subject, with a view, if not to change the muriatic acid into a new substance, at least to satisfy myself of the particulars of its action.

The same may be said respecting muriatic acid.

Dr Priestley partially examined the effects produced on sugar by muriatic gas.

I was aware, that Dr. Priestley had observed when muriatic gas was passed through a solution of sugar it gradually acquired a brown colour and strong smell; but on passing a current of this gas through a moderately strong solution, I was convinced of the extreme slowness of the process. Besides,

Besides I did not observe the effects as he describes them, till heating the mixture, when it grew black, and carbon became deposited.

The liquid acid substituted instead of the gas.

On account of the slowness of the process I substituted the weak liquid muriatic acid of the specific gravity of 1·050 or 1·080 instead of the gas, having first satisfied myself by experiments of the analogy of the results*. In some former experiments on the action of the oximuriatic gas on sugar assisted by heat, I had obtained the same results, and drawn the same conclusions, as Mr. Chenevix, though his results and conclusions were at that time unknown to me, it being only lately that I saw them in the last edition of Dr. Thomson's System of Chemistry. Mr. Chenevix thinks, that the oxigen of the oximuriatic acid goes to the formation of the malic acid, which is produced during the action; but as the experiments detailed in this paper will prove, that the muriatic acid acts with facility on sugar, we can scarcely doubt, that, after all the oxigen has been given to the elements of sugar from the oximuriatic acid, the muriatic acid acts on the remaining sugar, being thereby partially decomposed.

Oximuriatic acid assisted by caloric has considerable action on sugar: it forms the malic acid.

Mr. Vauquelin overlooked the malic acid when he examined the action of the oximuriatic acid on sugar.

Vauquelin does not mention the formation of the malic acid when sugar is acted upon by the oximuriatic gas, but says, "that the solution possessed the properties of caramel or partially burnt sugar." I have often been at a loss what substance to ascribe this French name to, whether to a new product formed during the decomposition of sugar by heat, or to the fumes of the pyromucous or acetic acid, which are given off plentifully. But, if by caramel is meant partially burnt sugar, we may altogether discard this name from our chemical systems, and substitute the old name molasses instead of it†. Under certain circumstances that

* My reason for substituting the weak acid instead of the strong was, that, as the strong acid occupies considerably less bulk, no large quantity of sugar would be dissolved; for, when the sugar is added in large quantity, the acid becomes diffused through its pores by capillary attraction. There can be no doubt however, that the action of both is perfectly analogous.

† "Caramel. Saccharum percoctum. Drogue que les apoticaires préparent pour le rhume, qui consiste particuliérement en du sucre fort cuit." Encyc. Franç. Lat. et Ang. Lond. 1761. C.

 this

this substance is present is sometimes the case, though we do not raise the heat high; but that the malic acid exists in abundance there cannot be the least doubt, notwithstanding the opinion of such an able chemist as Mr. Vauquelin.

The acid was pure.

The muriatic acid used in all the following experiments was pure. It gave no indication of any foreign ingredients by the usual reagents.

Muriatic acid at a low temperature dissolves sugar without decomposing it.

SECT. I. 1st. 50 grs of muriatic acid of the spec. grav. 1·050 were added to 50 grs of loaf sugar at the temperature 45° Far. The sugar dissolved without effervescence. The taste of the solution was acid, though slightly saccharine. The original stiffness of the acid was somewhat increased, and its colour changed to that of a yellowish brown. Saturated with a solution of subcarbonate of soda, and evaporated at 212°, it gradually acquired the consistence of a sirup; and very pungent white vapours were given off, which condensed on the lid that covered the capsule. From their taste and smell they appeared to be the pyromucous acid. If the 50 grs above had been saturated with soda, the muriate would have weighed 14 grs. 14 grs of muriate of soda were mixed with 50 grs of sugar dissolved in water, and submitted in every respect to the same operation as the solution of sugar in muriatic acid; when exactly the same phenomena presented themselves as in the former case, viz. the mixture acquired a sirupy consistence, and towards the close of the evaporation emitted acetic fumes. Hence it appears, that this change takes place without free muriatic acid being present; of course this acid had no share in the decomposition. This change took place exactly the same when a solution of sugar was evaporated rapidly; from which I infer, that cold muriatic acid has no action on sugar, except as a solvent. Whether it be the water contained in abundance in the dilute acid, which dissolves the sugar, or in some measure the acid itself, it would not be very easy to decide.

Muriatic acid assisted by heat has considerable action on sugar.

If, instead of saturating the solution of sugar in muriatic acid with soda, we apply a slight heat for some time, the mixture becomes black, and carbon precipitates. To obtain all the products of this apparent decomposition, I made use of the following apparatus. A small retort was joined

Description of

joined to a receiver with two necks; into one neck the beak of the retort was inserted, into the other a glass tube, which terminated in a glass air holder filled with water. The tubes were fitted through corks into their respective necks, and luted perfectly air tight with bees wax, or with resin. By this apparatus I was enabled to ascertain whether any gas, except the air of the vessels, came over during the application of heat, at the same time that the air holder had not the inconvenience that a common plain tube, terminating under the pneumatic shelf, would have had of admitting the water of the trough into the receiver, when impelled to it by the sudden condensation of the aqueous vapour in the retort. I shall now proceed to give the experiments as the facts presented themselves during the inquiry, being persuaded that this method is the most accurate, as well as the most concise.

the apparatus made use of.

SECT. II. 1st. 100 grs of muriatic acid, spec. grav. 1·080, were mixed with 100 grs of loaf sugar. A solution of the sugar was effected, accompanied by the emission of a slight pungent vapour of muriatic acid. This solution being introduced into the retort, and joined and luted to the auxiliary apparatus before described, the heat of 180° F. was applied by means of a water bath for half an hour. After about ten minutes had elapsed, abundance of carbon became deposited, and adhered to the bottom and sides of the retort firmly; till finally the solution became apparently solid from the copious deposition of carbon. During this change, not the least quantity of gas came over, except the air of the vessels, which returned again on suffering the apparatus to cool.

Heat applied to the solution of sugar in dilute muriatic acid precipitates carbon.

No gas came over in the process above.

2d. The liquid found in the receiver weighed 7 grs; of course a large portion must have adhered to the carbonaceous matter in the retort. But in subsequent experiments, on using a retort that exposed a larger surface of the liquid to evaporation, I have known it amount to 70 or 80 grs, though only exposed to heat the same time. Still, whatever may be the quantity which comes over, it always consists of two acids, the muriatic and pyromucous, or rather the acetic a little modified. If to this liquid we add carbonate of lead, an effervescence is the result, mu-

The liquid distilled into the receiver consisted of pyromucous acid and the muriatic.

riate of lead falls down; and by employing a close filter we may separate the insoluble muriate from the acetate, which passes through the filter. By saturation with soda the oxide is precipitated, and by evaporating the mother water we obtain the acetate, or at least the apparent acetate, dissoluble in rectified alcohol. The acid, which holds the lead in solution, appears more susceptible of being driven off by heat than the acid of the common acetate; for I have several times observed, that, when a solution of lead in it is concentrated by evaporation, a pungent smell is given off, and a yellow oxide is precipitated.

A partial solution of the substance in the retort effected.

3d. The residuum in the retort was detached by 1000 grs of water, added in quantities of 100 grs at a time, and employing some agitation. A partial solution of this substance was effected.

Carbon not pure.

4th. The substance insoluble being separated by the filter, it appeared to be carbon, though when heated it gave off gaseous inflammable matter. I have seen some sorts of impure charcoal do the same, under similar circumstances*.

The solution possessed the properties generally of muriatic and malic acid.

5th. The solution, which passed through the filter, was of the colour of red wine: its taste was acid, and it reddened vegetable blues. The various reagents generally adopted by our most eminent chemists did not indicate the presence of any of the following acids, viz. the gallic, oxalic, tartaric, and citric; neither did the benzoic, suberic, succinic, or camphoric exist in it. The only products, beside muriatic acid and a little undecomposed sugar, were a large quantity of malic and a trace of the acetic acid. To a known quantity carbonate of lead was added to saturation; the malate and muriate of lead were separated by the filter, and the acetate passed through. The same evidence of the presence of this acid was obtained as in sect. II. The substance left on the filter was of a brown colour. After

A small portion of the acetic likewise detected, or rather the pyromucous.

* The whole weight of this substance when perfectly dry would be about 36 grs. 10 grs, being heated red hot for some time, lost in weight 4 grs. Therefore 36 grs would lose 14·4; so that, if we could take into account all the carbonate in the product drawn off by heat, the quantity would probably differ little from the statement of Lavoisier, viz. in 100 parts 28 grs.

being

being well washed, a small quantity of dilute sulphuric acid was poured upon it. The mixed sulphate of lead and muriate were separated by the filter. What passed through possessed the original brown colour, and in part the acid taste; and had the properties of the malic acid, though it was evidently mixed with a small quantity of the muriatic. It is very difficult to separate these two acids from each other, without resolving the malic into its ultimate elements; the reagents being acted upon by each somewhat alike. In the above case the acetate of lead precipitated both the acids; and the sulphuric acid acted not only upon the malate, but also on the muriate: consequently instead of finding the malic acid singly, a mixture of the malic acid and muriatic were found. A fact which at first appeared somewhat puzzling to me was, that, on introducing a quantity of this fluid into a retort, and gently distilling, a large quantity of acid was found in the receiver; which, examined nearly by the method just mentioned, appeared to be of the nature of the acetous. If we apply heat a long time to the carbonaceous matter, which is plentifully deposited during the distillation, so as to drive off all the adhering acid; on macerating the dry mass in water we do not find a solution of malic acid, but sometimes, under certain circumstances, something of the remains of sugar*. This curious change is owing to the presence of muriatic acid, as the following comparative experiment will prove.

Curious fact: some of the liquid being distilled deposited carbon, and yielded pyromucous acid as a product.

I prepared some tolerably pure malic acid by bruising the leaves of the sempervivum tectorum (houseleek) along with a little water in an earthen mortar. The juicy mixture thus obtained contained a considerable portion of malate of lime. To remove the lime from the malate, a solution of oxalic acid was added cautiously, and the small excess was removed by lime water. The oxalate of lime was separated by a filter, and the liquid evaporated, till it became sufficiently concentrated. About a drachm of it was

Preparation of malic acid from the plant houseleek.

Malic acid distilled by itself does not deposit carbon or

* By returning the acid product into the retort, and distilling successively several times, this substance gradually disappears altogether, and the products are the acetous and muriatic acids and carbon.

yield pyromucous acid, but does when distilled with muriatic acid.

was introduced into a very small retort, and gradually distilled to dryness; no carbon became deposited, nor was any acid distilled into the receiver. The dry mass was again dissolved in water, and again distilled along with a few drops of muriatic acid; abundance of carbon now precipitated, and acetous acid was the product found in the receiver along with the muriatic.

Action of the muriatic acid on sugar something analogous to the action of the nitric; some element must be furnished.

As it would be absurd and vague to suppose such decompositions as the above could possibly take place without some new substance being furnished, and as the caloric would have been quite insufficient had not muriatic acid been present, we must of consequence suppose, that this acid is a compound body, capable of furnishing something analogous to that furnished by the nitric acid to sugar in similar situations; for the nitric in fact beside the oxalic forms a portion of malic acid, the quantity of which depends on circumstances. In some cases instead of finding oxalic acid I have found nearly the whole product malic acid, at the same time that something like carbon was deposited*. But if a part of the muriatic acid is furnished, to cause the elements of sugar to be differently arranged, of course it must be decomposed; that is, it must be reduced into its primary elements. The following fact is analogous: when the nitric acid changes the sugar into the oxalic acid, oxigen is furnished, and the other element, the azote, is given off in a combination with a smaller portion of oxigen, in the form of nitrous gas. This analogy would lead us to suppose, that to change sugar into the malic acid, at least some part of the muriatic must disappear, and enter along with the gasseous elements into the composition of the products, viz. the malic and acetous acids; not indeed in the form of muriatic acid, but in the form of some of its primitive elements. But before we can say much more on this subject, we must obtain positive evidence of its partial disappearance, because without such evidence, a nearly similar explanation of the above fact might be given, as that which Mr. Kind gave when he observed the change, that oil of turpentine underwent when acted upon by muriatic

Some part of the muriatic acid must disappear and be decomposed.

* In this case I cannot answer for the purity of the acid. The nitric of commerce sometimes contains muriatic acid.

gas,

gas. But if we obtain such evidence, then it at once follows, that this acid is a compound body; and that its disappearance, when made to act upon sugar, is owing to its ultimate decomposition. To ascertain this important point, after adopting several methods, I was led finally to pitch upon the following as the most susceptible of accuracy. The apparatus made use of for this purpose differed from the former only by a substitution of a tubulated retort for a common one.

Sect. III. 1st. One hundred grains of muriatic acid of the spec. grav. 1·050 were poured upon 50 grs of dry sugar, previously weighed and introduced into the retort. The apparatus was joined, and found to be perfectly air tight. After the sugar was dissolved, heat was applied to the retort, till about 90 grains of liquid were distilled over into the receiver. After the apparatus had become cool by several hours standing, the 90 grs just mentioned were poured back upon the carbonaceous matter in the retort, and again distilled in this manner five times, till finally heat was applied to the retort several hours, to drive off all the adhering acid. Care was taken in all this operation not to disjoin the apparatus, till it had been cool for some time, lest some vapour might rush out, and falsify the results. No extra gas passed over into the air holder, nor had the least sensible quantity of muriatic gas become condensed by its water, for it afforded no muriate with nitrate of silver.

2d. The liquid condensed in the receiver weighed 128 grs. Its colour was a reddish brown: its taste extremely acid: its smell that of aromatic vinegar nearly.

3d. The substance in the retort was tasteless. Water dissolved no part of it, but acquired an acid taste from a number of drops condensed in the neck of the retort*. The whole was thrown upon a filter to separate the carbon, which weighed, after being well washed and dried at 170° or 180° for some time, 18 grs. The liquid, which passed through, weighing 550 grs, gave a precipitate with sulphate of silver weighing 1·375.

* It contained neither a trace of malic acid nor a vestige of undecomposed sugar. The successive distillations having been with the presence of muriatic acid capable of decomposing both.

4th,

Proof that the acid in part disappears by comparing the weight of muriate of silver yielded by the original acid with that yielded by the same quantity of acid after having decomposed sugar.

4th. Ten grains of the original muriatic acid gave a precipitate with sulphate of silver, which weighed exactly 7 grs after having been dried perfectly on the vapour bath at 170 or 180 degrees. After this rate 100 grs, the quantity used in the experiment above, should yield exactly 70 grains dried at the same heat. 10 grs of the liquid (2d) gave with the same solution of sulphate of silver 4·937 grs dried the same exactly; therefore 128 would have given 63·194 nearly, which, added to the quantity of muriate of silver yielded by the 550 grs of liquid (3d), makes the whole amount of muriate of silver 64·569; which subtracted from 70 grs, the weight that would have been obtained had we operated on the original acid, leaves for deficiency 5·431. According to Dr. Marcet 100 grs of dry muriate of silver contain 19·05 of acid: taking this datum, 5·431 will contain 1·034, which is obviously the loss of real acid. I am at a loss to know, what objections may be brought against this experiment: for my part I can at present see none. The greatest care was taken, that no acid vapour might be lost in the various openings of the apparatus; and I have reason to believe, that not the least escaped, for the weight of the distilled product, which was 128 grs, compared with the few drops of liquid, that remained in the retort, made up along with the carbon the weight of the substances introduced. The muriate of silver in both cases was I think equally dried: both specimens were brought to the greatest state of dryness, by being exposed to exactly the same heat, and particular precaution was taken to bring each to the same state directly before being weighed. It gave me not a little uneasiness to obtain results, that would in any respect militate against the prevailing theory of sir Humphrey Davy. The last experiment I repeated several times with the greatest care, and I always obtained results little differing from the above. From their constant uniformity I cannot conclude less, than that a part of the acid disappears. To explain the rationale of the above fact I had first recourse to the present prevailing theory proposed by sir H. Davy, which supposes the muriatic acid to be compounded of hidrogen and chlorine gas; but from facts directly to be detailed I found it incapable, at least without bordering too much upon hypothesis.

In the experiment (sect. III) the new substances produced during the decomposition were a quantity of the pyromucous acid and water. I endeavoured to ascertain their relative proportions to each other by proceeding on the data of I think Vauquelin, that the pyromucous acid differs only from the acetic in being combined with an oil: though I did not succeed, being persuaded from several facts, that it either differs much from the acetic in composition, or otherwise that errour attends the analysis of the acetic acid by Dr. Higgins. I however saw evidence of the production of water to a considerable amount; and I can entertain but little doubt, that the pyromucous acid consists of oxigen, hidrogen, and carbon, though we do not know its absolute composition. To explain the above facts on the basis of sir H. Davy's theory, we must in the first place suppose, that hidrogen must be furnished to sugar to form the malic acid and the pyromucous, and that the other component part of muriatic acid, the chlorine, must be given off in the gaseous state of oximuriatic gas. But this explanation is insufficient, for the most delicate test that I could apply did not discover a trace of this gas. I am aware however, that a small quantity might adhere along with the muriatic acid insensible to our most delicate tests, as is certainly the case with the ordinary muriatic acid of commerce; but the quantity, which according to sir H. Davy we should have a right to expect, could not from its magnitude have operated in this manner. In the second place it might be supposed, that both these substances were furnished, viz. the chlorine gas and hidrogen: but this supposition would not in the least tally with the known component parts of the water and pyromucous acid, the new products. Sugar is composed of oxigen, hidrogen, and carbon; and the products of the decomposition are composed of the same substances, differing only in the relative proportions of their component parts. Hidrogen or oxigen indeed might have been furnished, but no other substance differing from these was furnished, nor could be furnished without forming a quaternary compound, which we are at present not acquainted with*. The excess of ingredients in this decomposition being only in oxigen and hidrogen, and

The whole products of the last decomposition of sugar were water and pyromucous acid.

Sir H. Davy's theory of the compound nature of the muriatic acid insufficient to explain the rationale of the above facts.

The strictest analogy would lead us naturally to suppose, that the muriatic acid is composed of oxigen and hidrogen.

* Consisting of oxigen, hidrogen, chlorine, and carbon.

as no gaseous matter whatever escaped, must we not suppose, that both component parts of the muriatic acid which disappeared entered into the composition of the two products, water and pyromucous acid? If only one entered, the other would be given off; but this was not the case, for no gas whatever, as I have shown before, was produced; of consequence we may I think conclude, that muriatic acid is composed of oxigen and hidrogen.

Reasons for making the oximuriatic acid compound.

Upon strict analogy we cannot conclude less than that the oximuriatic gas or chlorine gas of Davy is a compound. This when heated along with sugar forms malic acid even in more abundance than the muriatic acid does. The malic acid, when submitted to heat capable of decomposing it into its elementary principles, gives us an acid (the pyromucous), water, a large portion of carbonic acid, and some carburetted hidrogen. Hence it must be composed like sugar of oxigen, hidrogen, and carbon: consequently the malic acid is of known composition. If the chlorine gas was simple we could not obtain bodies the composition of which is known, and in which no such principle is found. Instead of obtaining malic acid, which is a ternary combination, we should have obtained of course a quaternary compound, or a direct compound of oxigen, hidrogen, carbon and chlorine; which would have been a body unknown to us, or a new substance. If I can in any degree draw the attention of your more able correspondents to this subject, so as to enlarge more upon it, my sole aim will be fully answered.

Farnley Wood, near Huddersfield, I. N.
June the 10*th*, 1812.

IX.

On the Zig-zag Motion of the Electric Spark. In a Letter from I. A. De Luc, Esq. F. R. S.

To W. NICHOLSON, Esq.

Sir,

Papers in the Journal on electricity.

I have found in your No. 144, two papers, on which I shall take the liberty of communicating to you some remarks: one

one is Art. II, signed J. PHŒNIX, concerning the *zig-zag motion of the electric spark*; and the other Art. XI, by DR. MAYCOCK on *the production of electrical excitement by friction*, which is the continuation of another in your No. 131. These papers concerning *electricity* have strongly excited my attention, as you may suppose from my papers on the same subject in your Journal. But for the present I shall confine myself to the paper signed J. PHŒNIX, on the *zig-zag motion of the electric spark*.

The author says (p. 243), "that this subject seems to "have been withheld entirely from public discussion." But he immediately mentions the true explanation of this phenomenon in the following manner. "The only account I "have heard in lectures was, that by its own *rapidity* of "motion it *condensates* the *air* to such a degree, that it has "to move, as it were, from a *solid* to a less *dense medium*; "which seems to me *impossible*." I shall first consider this rejected explanation with respect, not only to its *possibility*, but to its *sufficiency*. Cause of the zigzag motion of the spark.

The *electric fluid* moves with a great *velocity*, as we may judge by the sight; and it is such, that we cannot estimate it, comparatively to that of *light*; but it is much *denser*, as we see by the *hole* that a strong *spark* produces in a card which is opposed to its course; it may therefore occasion a sufficient *compression* in the *air* before it, so that at last it is repulsed sidewise. Capable of compressing the air before it, so as to be repelled sideways.

We have an example of the *repulsion* in the *air* itself. The instrument called *anemometer* shows the *velocity* of the *wind*, because the *air* in motion, finding in it an obstacle, is *condensed* against it, and thus *presses* it forward; but if it finds less *resistance* on one side, it *escapes* and presses the obstacle *sidewise*. The immediate *pressure* of *air* is shown in the ingenious *anemometer* of Dr. LIND, described in the 65th vol. of the *Philos. Transactions*, p. 363. This instrument consists of a glass siphon, having quicksilver in both its branches, open at their extremities, one of which is bent forward at a right angle. When the siphon is held upright, and the opening of the bent branch is turned towards the *wind*, the quicksilver is *depressed* in it, and *ascends* in the other, in proportion to the *velocity* of the *current of air*. Example of this in the air.

As

Lateral pressure of air on the sails of a ship.

As for the *lateral pressure of air*, when it experiences *less resistance* on one side of its course than on the other, we have an example of this effect in the motions of *ships*; why do they change their course by the different *inclination* of their *sails?* It is because they offer *less resistance* to the motion of the *air*, which thus changes its course; however it *presses sidewise*, so as to put the *ship* in a different motion, which is determined by the rudder. This is an example absolutely analogous, only inverse, of the change of course of the *electric spark*; this compresses the *air*, until finding *less resistance* on one side, it suddenly changes its course.

Erroneous hypothesis.

I come to the author's hypothesis, in which he sets out from this certain fact; "that the *electric fluid* passes in a "more direct line according to the best or the worst *conducting* substances presented to it:" but not being sufficiently conversant with *meteorological* phænomena, he makes an hypothesis, which will give me the opportunity of showing how necessary is their knowledge in every branch of experimental philosophy, to avoid arbitrary, and even delusive hypotheses. "Our atmosphere," he says, "being a compound "of *oxigen*, &c. presents at once, to the *spark*, flying from "the machine, at least four known *gasses*; all, *I have not* "*the smallest doubt*, differing in their *conducting* power, *were* "*they separately tried*." This therefore remains a mere *hypothesis*, till the *trial* has been made; however he thus continues: "*This point being ascertained*, the phenomenon is "at once accounted for. The fluid flies to the next *best* con- "ducting *gas* from a *worse*, as it would from different por- "tions of matter."

Importance of meteorological phenomena in science.

I hope the author will see now, that he has not *accounted* for this phenomenon. But, Sir, he himself, or others of your readers, will I hope take some interest in a short account of the *meterological* phenomena, which might have prevented his hypothesis, in the first class of which are the following.

Nature of the atmosphere.

I have proved in my work *Idées sur la Météorologie*,—1. That it is an errour to consider the principal mass of the atmosphere as composed of two distinct fluids, or *gasses*, one called *oxigen*, the other *hidrogen*; that *atmospheric air* is a fluid *sui generis*, composed, in each particle, of all the ingredients manifested in its *decompositions*.—2 That *atmospheric*

air

air is a transformation of the *aqueous vapour* which constantly ascends in the atmosphere.—3 That *rain* is produced by the *decomposition* of that *air*, which returns to *aqueous vapour*, first in *clouds*, from which, by their condensation, *rain* proceeds.

Different hypothesis of rain inconsistent with facts.

Those among natural philosophers who have not adopted this system, being however obliged to explain the production of *rain*, have supposed that the *aqueous vapour*, ascending in the atmosphere, accumulates in its upper parts, where it is condensed by the *cold* of that region. But in the first place it has been found by Mr. DE SAUSSURE, and myself, by *hygroscopical* observations, that the more we ascend in the atmosphere, the less of *aqueous vapour* is mixed with the *air*. Besides, from this hypothesis, *rain* should fall only in the *night*, when the atmosphere *cools* after sunset. But the spring of this year has furnished a test to the atmospheric systems. We have had almost incessant *rains*, with great *storms*. Where could that enormous quantity of *water* be contained, if not in the composition of the air itself?—What could have occasioned these tremendous local *storms*, except the decomposition of *air* in certain *extents*, toward which the other *air* was rushing?

Another objection to the author's hypothesis.

However this analysis of the constitution of the atmosphere is not necessary to show how groundless the author's hypothesis is; for it is a known fact, that if such distinct *gasses* as *oxigen* and *hidrogen* exist in its mass, they are no where *separated* in the whole of its extent, from the plain to the top of the highest mountains: consequently the *electric spark* can no where be attracted on one side more than another, even were it proved that these *fluids* possess different *conducting* faculties. Therefore there remains only the explanation which the author rejects, because he was not informed of these facts.

Falling stars.

There is a phenomenon, which shows to the sight the manner in which some *fluids*, distinct from *atmospheric air*, ascending in the atmosphere, follow their course; I mean what is called *falling stars*, when they follow a long track. This is a *phosphoric* fluid, ascending from some spot of the surface of the Earth. It is invisible in its ascent, because there is some circumstance required to make it *phosphorescent*,

cent, by decomposition; but when this happens, the *light* disengaged makes it visible the whole way, and this is in a *straight line*. The small *falling stars* are composed of the same *fluid*, but it has been disturbed in its ascent by the agitation of the air; its *streams* have been divided, and their *direction* changed.

If the author has any objection to the whole, or to some part of this answer to his system, I shall be glad to receive it through your Journal; but he will find, I think, that it involves many more objects of *meteorology* than he was aware of; as this is connected with most part of natural philosophy,

I remain, SIR,

your most obedient servant,

J. E. DE LUC.

Windsor, June the 18*th*, 1812.

X.

Remarks on an artificial stony Substance: *by* F. R. CURAUDAU*.

Solidification of water.

A Remarkable example of the high degree of solidification that water can acquire in certain combinations is exhibited by the artificial stones, which form the subject of the present remark.

Composition of an artificial stone.

These stones, more than half the weight of which is water, consist also of sulphuric acid and baked clay reduced to powder, in the proportion of one part of the former and two of the latter. The simple mixture of these three substances affords only a solution of sulphate of alumine: but, if their mutual action be promoted, heat is soon produced, and its evolution is sometimes so considerable, that the matter seems incandescent.

Action of a large quantity of materials.

If the quantity of materials amount to 25 or 30 hundred weight, this beautiful phenomenon lasts above an hour. But, what is particularly remarkable, if the matter come to want water at the moment when the mutual action of the

* Journ. de Phys. vol. LXVIII, p. 409.

substances

substances on each other is most energetic, the mass, though still fluid, acquires suddenly a great degree of solidity ; the heat is even increased in its intensity ; and the matter afterward passes almost wholly to a state of insolubility. The latter property, acquired by a mixture intended to produce very soluble salts, proves, that the penetration of the earth by the water and acid must have been very great, since the whole mass forms only a stony compound.

A stone similar in appearance, but not insoluble.

The stones to which I here allude, though having in appearance all the properties of those I have just described, have not the quality of being insoluble. On the contrary I prevent their passing to this state, as then I could not make use of them. But as this compound has all the external characters of the hardest stones, except that it is not insoluble, I conceived it would not be uninteresting to see an artificial stony substance, which some peculiar properties might render useful. For instance, as it may be softened by a heat superior to that of boiling water, might it not be employed with much advantage for fastening iron or wood in stone, casting statues, moulding vases, and many other purposes, that experience would point out ? It is true that substances formed of this stony paste must not be exposed to wet.

Applicable to different uses.

Cause of volcanic eruptions,

Another consideration, that has led me to suppose this new stony compound would not be viewed with indifference, is, that the theory of its formation, and its analogy with the stones of solfaterras, render it unnecessary for us to have recourse to the hypothesis of subterranean fires kept up by combustible matters, to explain the eruptions of volcanoes.

In fact, since water alone, by passing instantaneously from the liquid to the solid state, can give rise to the evolution of so very considerable a degree of heat, may it not be the immediate cause of volcanic eruptions ? Is it not likewise the slow and gradual passage of water to the solid state, that produces the heat kept up at great depths in the interior of the globe ? Lastly, is not the heat developed in animal and vegetable organization equally owing to water ?

interior heat of the Earth,

and organic bodies.

SCIENTIFIC

SCIENTIFIC NEWS.

Society for the Encouragement of Arts, &c.

Society of Arts, &c. Premiums for planting forest trees.

In the year 1808, the gold medal of the Society of Arts &c. was adjudged to Dr. Bain, of Curzon-street, for planting 338199 forest trees, at Heffleton, in Dorsetshire, in 1804 and 1805. These were part of more than eight hundred thousand, that he had planted from 1798 to that time on a heath valued to the tenant at 1s. an acre per annum, a poor gravelly soil, on a situation rather elevated, and much exposed to the winds from the seacoast. Thus encouraged, and the trees for the most part thriving well, the Dr. has pursued his exertions, adding near three hundred thousand trees more to his plantations, on ground not adapted to the purposes of husbandry. The trees are chiefly larch, pinaster, and Scotch fir; the last in much the largest proportion. The luxuriance of his pinasters in particular show the propriety of planting them as a shelter to other trees. The following table shows the size attained by some of the pinasters and larches in twelve years after planting. The pinasters were seedlings of one year old, planted on very poor ground; the larches were three years old when planted, and the land of a better quality.

		CIRCUMFERENCE.						HEIGHT.	
		at the ground		3 ft. from grd.		6 ft. from grd.			
	No.	ft.	inch.	feet	inch.	feet	inch.	feet	inch.
Pinaster.	1	3	0	2	4	1	10	20	0
	2	2	8	2	0	1	4	17	0
	3	2	5	1	10	1	4	18	3
Larch.	1	3	0	2	0	1	7	24	6
	2	2	6	1	9	1	6	23	9
	3	2	5	1	8	1	5	23	11

For these plantations a second gold medal was adjudged to Dr. Bain this session.

Premiums for oaks.

The gold medal, being the premium offered in class 3 for raising oaks, was adjudged to Henry Andrews, Esq., of Wakefield; and, in consequence of the death of Mr. Andrews, the medal was presented to his two daughters. The oaks were planted with other trees, and the following is an account of the whole.

In

	In February and March, 1809.	In Feb. and March, 1810.	Total.
Black Italian poplars	500	1000	1500
Huntingdon willows	1000	1000	2000
Ash	6000	5000	11000
Oaks	12000	10000	22000
Scotch firs	45000	48000	93000
Birch	6800	8000	16800
Larch	10000	20000	30000
Spruce	10000	20000	30000
Alders	1800	10000	11800
Sycamores	660		660
	95760	123000	218760

The first plantation was 36 acres, 3 rods, 10 perches; the second, 42 acres, 1 rod. The whole is well fenced with sod walls, five feet high, and three feet and half thick.

Premium for larches.

The gold medal was also adjudged to Wm. Congreve, Esq., of Aldermaston house, Berkshire, for planting 377520 larches, being the premium offered in class 10. He planted 108 acres in rows 3 feet asunder, and the plants at the same distance: 50 acres with the trees six feet asunder each way, except near the outsides, where they were only three feet; and 32 acres with the trees four feet distant each way, which distance he thinks preferable to any other. It is his intention to extend his plantations to 500 or 600 acres. Several of the last years shoots of a small plantation of larch, made in 1806, exceeded three feet in length, and one was three feet nine inches.

Second premium for larches.

The silver medal was voted to Mr. Henry Cowlishaw, of Mansfield, for planting 75000 larches, being the premium offered in class 11. The land is on Blidsworth forest, part of Sherwood. The following account is in his own words.

Management of the plantation.

The land being chiefly covered with heath from six to eighteen inches high, I caused a piece of the heath sod to be pared off with a paring-spade, of a sufficient space to plant the tree in; and the soil being very thin and near the gravel, I preferred planting the tree without turning over the soil.

The season being far advanced, and not having been sooner in possession of the land, I ordered that the roots of the trees should be made wet with water, and then rubbed over

Plantation of larches.

with soil, which thus adhered to the roots; and in this state they were planted in the proportion of rather more than five thousand trees upon each acre, having planted seventy-five thousand trees upon the land, which is not more than fourteen acres, allowing for the fences.

The larch trees were two years transplanted, and from eight to fifteen inches high when planted out.

The season proved very favourable, few of the trees died, as one thousand filled up the deficiencies in the autumn of 1808, and the remainder grew well. In the autumn of 1809 they were again filled up with the same number; and I have this month supplied all the deficiencies with two thousand more, as some had been destroyed by rabbits.

The plantation is now in a healthy growing state; the last season it has much improved.

I think the above mode preferable either to destroying the heath, (as I presume it preserves the moisture in the soil during the summer, and affords warmth in the winter), or making holes by turning up the soil, and bringing what is bad upon the surface.

I am justified in these remarks from plantations adjoining mine, where both modes have been tried, and neither has answered so well as my method. My plantation is protected by a quick fence, which was planted in 1808, and secured by good posts and rails all round; the quicks have grown very well, considering the nature of the soil, which is but barren, and they are likely to become a good fence.

The following is an account of the expences that have attended this plantation.

	£	s.	d.
Purchase of the land and stamp	200	16	0
Seventy-nine thousand larches at £1. per thousand	79	0	0
Posts and rails..............................	30	9	0
Paring, planting, and putting down the fences	38	0	0
Carriage of trees, &c.......................	2	16	0
Cleaning the trees first and second year, where the heath in any measure incommoded them	2	11	0
Expenses of filling up the deficiencies	3	0	6
	356	12	6

Wernerian

Wernerian Natural History Society.

At the meeting on the 28th of March, professor Jameson read an account of a floetz gypsum formation, which occurs on the banks of the Whitadder, near Kelso. Likewise of a beautiful floetz quartz found in beds in the coal district of Fifeshire: and of the occurence of basalt, amygdaloid, and trap-tuff, in a coal-formation, newer than the old red sandstone, and its accompanying porphyry, but probably older than the general mass of the rocks of the newest floetz-trap formation. At the same meeting, Mr. Leach read a description of the pig of Orkney and Shetland, which he is inclined to consider as a distinct species. And the Secretary laid before the meeting a very full and interesting thermometrical register and meterological journal, kept on a voyage to Davis Straits and back again, by Mr. John Aitkin, surgeon. Mineralogy. Species of pig. Meteorological journal.

At the meeting on the 11th of April, Dr. Macknight read a mineralogical description of Tinto, a noted mountain in Lanarkshire. It appears to be of floetz formation; probably resting on the gray wacke, which pervades the whole mountainous districts in the south of Scotland. Around the base is found conglomerate, containing rounded masses of gray-wacke, iron clay, flinty slate, splintery hornstone, quartz, felspar, mica, &c. Where the rock becomes finer grained, it approaches in some places to gray-wacke, and in others to those portions of the old red sand-stone formation, which are conjectured to alternate with the newer members of the transition series. Over the conglomerate, masses of clay-stone, greenstone, and greenstone passing into clinkstone, and porphyry-slate, successively appear, till we reach the summit, which, along with the whole of the upper part, is found to consist of compact felspar, and felspar porphyry. The disposition of the rocks in this mountain is conformable to the idea of secondary deposition, by assuming a finer and more crystalline texture as they ascend; and the occurrence of claystone and felspar in a position corresponding to what is observed on the Eildon Hills, the Pentlands, the Ochills, Papa Stour, Dundee, and in other places, seems to favour the hypothesis of a particular overlying formation, in which Mountain of Tinto.

these

these substances are prevailing ingredients, extending over a considerable portion of the lower country of Scotland.—In the bed of the Clyde, to the eastward of Tinto, amygdaloid appears, having nodules of calcedony coated with green earth; also calcspar, and portions of steatite.—Towards the north, the conglomerate forming the base of Tinto passes into the sandstone of which the whole inferior districts of Lanarkshire are composed. It is to the waste of this rock that we owe the splendid scenery of Cora Linn, and the other celebrated falls of the Clyde, a river which exhibits in its course many charms of nature, and may indeed be said to carry along with it beauty and fertility.

Meteorology of Hudson's Bay.

At the same meeting, the Secretary communicated a very curious meteorological journal, kept by Governor Graham, during his residence in Hudson's Bay.

Geological Society.

Substances distilled from wood analogous to bitumens.

May the 1st. A paper by Dr. Mac Culloch, M. G. S., on bistre and other substances produced in the distillation of wood; and on their analogy with the native bitumens, was read. When wood is submitted to destructive distillation, there is obtained, among other products, a black substance resembling common tar. This tar is very inflammable, and so liquid, that it may be burnt in a lamp. By washing it with water either hot or cold, or submitting it to the action of lime, or of the mild alkalis, a large portion of acetic acid is separated, and the residue becomes pitchy and tenacious. It is entirely soluble in caustic alkali, in alcohol, in ether, in acetic acid, and in the mineral acids. The fat oils and the recent essential oils dissolve but little of it, but if the former are made drying, and if the latter have become brown by keeping, they then act more readily and copiously. Coloured oil of turpentine takes up a considerable quantity, but naphtha only acquires a scarcely sensible brown colour, by digestion upon it. When carefully distilled at a gentle heat it is decomposed into an oily matter, at first limpid, and afterward brown, a quantity of acetic acid combined with a little ammonia, and a spungy coal remains in the retort. In this process

process no inflammable gas is given out; but at a high temperature the oil is more or less decomposed, and inflammable gas is produced; which, however, does not burn with a flame by any means so bright as the gas from pit coal.

Residuum.

If this destructive distillation is not carried very far, the matter in the retort will be found, when cold, to be solid, brilliant, shining, and possessed of a conchoidal fracture: its taste is burning and pungent, and its odour is that of wood smoke. It is fusible and readily inflammable. When kept melted in an open vessel, till it ceases to be fusible, it becomes more and more brilliant, its fracture passes to splintery, and it assumes the perfect appearance of asphaltum. In proportion as it approaches this state it becomes less and less soluble in alcohol, and at length scarcely gives a stain to this mentruum. Naphtha has no action on it, and in this circumstance alone it differs from asphaltum.

Difference between bitumens and recent vegetable matter.

Dr. Mac Culloch then proceeds to an examination of the bitumens, and shows, that the difference between the products of recent vegetable matter and of the bitumens, when subjected to distillation, consists in the former yielding empyreumatic acetic acid, and a black pitchy matter insoluble in naphtha; while the latter afford ammonia and naphtha, but little or no acid.

Lignites examined.

He then enters into a detailed investigation of the properties of the very important class of lignites, or those substances such as peat, surturbrand, Bovey coal, &c. in which the traces of vegetable origin are not obliterated. Submerged wood from peat mosses gave a brown oil, smelling of wood tar, and refusing to dissolve in naphtha. A compact pitchy looking peat gave a fetid oil, resembling in odour neither wood tar nor bitumen, and very slightly soluble in naphtha. Bovey brown coal gave an oil resembling in odour that of wood tar, but much more soluble in naphtha. That portion of the oil which was insoluble in this menstruum had a strong odour of wood smoke. The oil of jet was almost perfectly soluble in naphtha, and smelled strongly of petroleum, but it afforded also empyreumatic acetic acid.

Thus

Wood changed to bitumen by water.

Thus it appears, that there exists a class of fossils of undoubted vegetable origin, which exhibit the gradual progress from wood to bitumen, and in which this change has been brought about by the action, not of heat, but of water.

Experiments of Sir James Hall.

The experiments however of Sir James Hall seem to show, that heat with compression is also capable of converting wood into coal. A critical examination of this fact was the next object of Dr. M., and he found on heating wood in close gunbarrels, that a black coaly looking subsubstance was indeed produced, but that it consisted wholly of charcoal, empyreumatic acid, and wood tar; and did not contain the smallest portion of real bitumen: hence the experiments alluded to do by no means prove the possibility of converting vegetable matter into real coal by mere heat. It appears, however, to Dr. M., that the consolidation of bituminized vegetables into coal is not unlikely to be the effects of subterranean heat.

Bistre the pitch of wood.

Improvement and uses of it.

This paper concludes by showing the identity of the pitch procured from the distillation of wood and the pigment called bistre; points out methods of obtaining it in a state better fitted than common bistre for the purposes of the artist; and also enumerates several other uses, to which this substance may be economically applied.

Mineralogy of St. David's.

Some notes on the mineralogy of the neighbourhood of St. David's in Pembrokeshire, by Dr. Kidd, Prof. Chem. at Oxford, and Hon. M. G. S., were read. The Country about St. Davids, when viewed from an eminence, presents the appearance of an extensive uneven plain, interspersed with numerous detached hills or rocky summits of an irregular conical shape. The two highest of these hills are Penberry and Carn-Llidy, the western portion of the latter of which forms the promontory of St. David's head. These hills present no appearance of stratification, and are composed of felspar and hornblend in various proportions and states of aggregation. They are each surrounded by mantle shaped strata of slate, elevated at a high angle, and presenting the characters of grauwacke slate; this latter is traversed by veins of quarts, from which very fine specimens of rock crystal are procured. No carbonate of lime

appears

appears to be contained either in the unstratified trap, or in the slaty grauwacke, nor did there occur in them, with the exception of one equivocal instance, the smallest trace of any organic remain.

Island of Teneriffe.

May the 15th.—An account of the Island of Teneriffe, by the Hon. Henry Grey Bennet, M. G. S. was read. The greatest length of this island from north to south is about 70 miles, its greatest breadth does not exceed 30 miles. In the S. W. part of the island is situate the mountain called by the Spaniards *el Pico di Tiede*, but better known by the name of the Peak of Teneriffe, the height of which, from the mean of several observations, appears to be about 12500 English feet. The rocks and strata of this island appear to be wholly volcanic. A long chain of mountains passes through the interior, sloping on the E. W. and N. sides to the sea, but on the S. and S. W. elevated into nearly perpendicular mountains, which are intersected by deep and narrow ravines. The lowest bed of the island is porphyritic lava, composed of hornblende and felspar, in its upper part porous, scoriform, and sometimes passing into the state of pumice. Upon this rests a bed of the same substance, as already mentioned, but in structure nearly approaching to greenstone. This is covered by a thick bed of pumice, which itself is overspread with basaltic lava, on which, in many places, rest beds of tufa and volcanic ashes. This basaltic lava decomposes sooner than any of the other rocks, and contains the greatest variety of imbedded substances: it is sometimes divided by a layer of olivine in crystals some inches long, and is often intersected by thick veins of porphyritic slate. Zeolite and chalcedony also occur in it. The number of small craters and extinct volcanoes is prodigious, They are to be found in all parts of the island; but none of them have been in activity of late years. The great streams of lava have flowed from the Peak: those of the years 1704 and 1797 (which was the last) are basaltic. This latter flowed so slowly, notwithstanding the steep descent of the mountain, that it was several days in advancing three miles. On the western side of the Peak is an ancient lava, not at all decomposed, several mi[illegible]ength, and in a perfect state of vitrification resembli[illegible]idian.

Mr. Vauquelin

Mineral water of Néris.

Mr. Vauquelin has analysed the thermal water of Néris, near Montluçon, in the department of the Allier. Two ounces of the solid matter left by evaporating the water on the spot had been sent to him: but he was not informed of how much water it was the produce. The results of his analysis were.

Carbonate of soda	33·34
Sulphate of soda	28·68
Muriate of soda	15·28
Carbonate of lime	2·80
Silex	8·34
Water	9·02
Animal matter, and loss	2·54
	100·

The silex he supposes to have been held in solution by the water; and he thinks it probable, that both this and the animal matter were indebted for their solubility to the presence of the carbonate of soda.

Mineral water of Argentières.

He likewise analysed the residuum of the water of Argentières, sent him in the same way by the same physician. The results were.

Carbonate of soda	32·08
Sulphate of soda	15·75
Muriate of soda	1·39
Siliceous sand	10·42
Carbonate of magnesia	34·37
———— lime	5·21
Animal matter	0·78
	100·

Mathematical Repository.

The Twelfth Number of Leybourn's Mathematical Repository contains—1. Solutions to the Mathematical Questions proposed in Number X. 2. On the irreducible Case of Cubic Equations. 3. New Properties of the Conic Sections. 4. Indeterminate Problems. 5. On the Ellipse and Hyperbola. 6. On the Roots of Equations of all Dimensions. 7. Properties of the Right-angled Triangle. 8. Continuation of Le Gendre's Memoir on Elliptic Transcendentals. 9. A series of new Questions to be answered in a subsequent number.

To Correspondents.

Dr. Henderson's paper is obliged to be postponed till next month.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1812.

ARTICLE I.

On a gaseous Compound of carbonic Oxide and Chlorine. By JOHN DAVY, *Esq. Communicated by Sir* HUMPHREY DAVY, *Knt. LL. D. Sec. R. S**.

SINCE the influence of electricity and solar light, as chemical agents, are analogous in many respects, and as the former produces no change in a mixture of carbonic oxide and chlorine, it was natural to infer the same respecting the latter. Messrs. Gay-Lussac and Thenard assert, that this is the case; they say, that they have exposed a mixture of carbonic oxide and chlorine, under all circumstances, to light, without observing any alteration to take place†; Mr. Murray has made a similar statement‡.

Oximuriatic gas said not to act on carbonic oxide.

Having been led to repeat this experiment, from some objections made by the last mentioned gentleman to the theory of my brother, sir Humphry Davy, concerning chlorine, I was surprised at witnessing a different result.

The contrary found by Mr. Davy.

The mixture exposed, consisted of about equal volumes of chlorine and carbonic oxide; the gasses had been previously

Experiment.

* Philos. Trans for 1812, p. 144.

† Recherches Phisico-Chimiques, Tom. II, p. 150.

‡ Nicholson's Journal, vol. XXX, p. 227.

ously dried over mercury by the action of fused muriate of lime; and the exhausted glass globe, into which they were introduced from a receiver with suitable stopcocks, was carefully dried. After exposure for about a quarter of an hour to bright sunshine, the colour of the chlorine had entirely disappeared; the stopcock belonging to the globe being turned in mercury recently boiled, a considerable absorption took place, just equal to one half the volume of the mixture, and the residual gas possessed properties perfectly distinct from those belonging either to carbonic oxide or chlorine.

Properties of the resulting gas.

Thrown into the atmosphere, it did not fume. Its odour was different from that of chlorine, something like that which one might imagine would result from the smell of chlorine combined with that of ammonia, yet more intolerable and suffocating than chlorine itself, and affecting the eyes in a peculiar manner, producing a rapid flow of tears, and occasioning painful sensations.

Its chemical properties were not less decidedly marked, than its physical ones.

Thrown into a tube full of mercury containing a slip of dry litmus paper, it immediately rendered the paper red.

Mixed with ammoniacal gas, a rapid condensation took place, a white salt was formed, and much heat was produced.

The compound of this gas and ammonia was a perfect neutral salt, neither changing the colour of turmeric nor litmus; it had no perceptible odour, but a pungent saline taste; it was deliquescent, and of course very soluble in water; it was decomposed by the sulphuric, nitric, and phosphoric acids, and also by liquid muriatic acid; but it sublimed unaltered in the muriatic, carbonic, and sulphureous acid gasses, and dissolved without effervescing in acetic acid. The products of its decomposition collected over mercury were found to be the carbonic and muriatic acid gasses; and in the experiment with concentrated sulphuric acid, when accurate results could be obtained, these two gasses were in such proportions, that the volume of the latter was double that of the former.

Decomposd into carbonic and muriatic acid gasses.

Condenses 4 times its bulk

I have ascertained by repeated trials, both synthetical and analytical, that the gas condenses four times its volume of the

the volatile alkali, and I have not been able to combine it with a smaller proportion. *of ammonia.*

Tin fused in the gas in a bent glass tube over mercury, by means of a spirit lamp, rapidly decomposed it; the liquor of Libavius was formed; and when the vessel had cooled, there was not the least change of the volume of the gas perceptible; but the gas had entirely lost its offensive odour, and was merely carbonic oxide; for like carbonic oxide it burnt with a blue flame, afforded carbonic acid by its combustion, and was not absorbable by water. *Decomposed by tin,*

The effects of zinc, antimony, and arsenic heated in the gas, were similar to those of tin; compounds of these metals and chlorine were formed, and carbonic oxide in each experiment was liberated equal in volume to the gas decomposed. In each instance the action of the metal was quick; the decomposition being completed in less than ten minutes: but though the action was rapid, it was likewise tranquil, no explosion ever took place, and none of the metals became ignited or inflamed. *zinc, antimony and arsenic,*

The action even of potassium heated in the gas was not violent. But from the great absorption of gas, and from the precipitation of carbon indicated by the blackness produced, not only the new gas, but likewise the carbonic oxide, appeared to be decomposed. *potassium,*

The white oxide of zinc heated in the gas quickly decomposed it, just as readily indeed as the metal itself; there was the same formation of the butter of zinc; but instead of carbonic oxide being produced, carbonic acid was formed; and, as usnal, there was no change of volume. *white oxide of zinc,*

The protoxide of antimony fused in the gas rapidly decomposed it; the butter of antimony and the infusible peroxide were formed; there was no change of the volume of the gas, and the residual gas was carbonic oxide. *and protoxide of antimony.*

Sulphur and phosphorus sublimed in the gas, produced no apparent change; the volume of the gas was unaltered, and its characteristic smell was undiminished. *Not decomposed by sulphur, phosphorus,*

Mixed with hidrogen or oxigen singly, the gas was not inflamed by the electric spark, but mixed with both, in proper proportions, viz. two parts in volume of the former and one of the latter to two parts of the gas, a violent explosion *or hidrogen or oxigen singly;*

was produced, and the muriatic and carbonic acid gasses were formed.

but quickly by water.

The gas transferred to water was quickly decomposed, the carbonic and muriatic acids were formed, as in the last experiment, and the effect was the same even when light was excluded.

Nature of the compound.

From the mode of the formation of the gas and the condensation that takes place at the time, from the results of the decomposition of its ammoniacal salt, and from the analysis of the gas by metals and metallic oxides, it appears to be a compound of carbonic oxide and chlorine condensed into half the space which they occupied separately.

Seemingly an acid.

And from its combining with ammonia, and forming with this alkali a neutral salt, and from its reddening litmus, it seems to be an acid. It is similar to acids in other respects; in decomposing the dry subcarbonate of ammonia, one part in volume of it expelling two parts of carbonic acid gas; and in being itself not expelled from ammonia by any of the acid gasses, or by acetic acid. Independant of these circumstances, were power of saturation to be taken as the measure of affinity, the attraction of this gas for ammonia must be allowed to be greater than that of any other substance, for its saturating power is greater; no acid condenses so large a proportion of ammonia; carbonic acid only condenses half as much, and yet does not form a neutral salt. The great saturating and neutralizing powers of this gas are singular circumstances, and particularly singular when compared with those of muriatic acid gas.

Its attraction for ammonia very great.

Its relation to the fixed alkalis not known.

In consequence of its being decomposed by water, I have not been able to ascertain whether it is capable of combining with the fixed alkalis. Added to solutions of these substances it was absorbed, and carbonic acid gas was disengaged by an acid.

It does not decompose carbonate of lime or barytes.

I have made the experiment on the native carbonates of lime and barytes, but the gas did not decompose these bodies. This indeed might be expected, since quick-lime, I find, does not absorb the gas: a cubic inch of it, exposed to the action of lime in a tube over mercury, was only diminished in two days to nine tenths of a cubic inch, and no farther absorption was afterwards observed to take place.

But

But even this circumstance does not demonstrate, that the gas has no affinity for lime, and is not capable of combining with it; for on making a similar experiment with carbonic acid, substituting this gas for the new compound, the result was the same; in two days only about one tenth of a cubic inch was absorbed.

Not decomposed by spirit of wine.

Though the gas is decomposed by water, yet it appears to be absorbed unaltered by common spirit of wine, which contains so considerable a quantity of water; it imparted its peculiar odour to the spirit, and its property of affecting the eyes; five measures of the spirit condensed sixty measures of the gas.

Absorbed by the fuming liquor of arsenic and oximuriate of sulphur.

It is also absorbed by the fuming liquor of arsenic, and by the oximuriate of sulphur.

The former appeared to require for saturation ten times its own volume; six measures of the liquor condensed about sixty of the gas. The liquor thus impregnated was thrown into water, and a pretty appearance was produced by the sudden escape of bubbles of the gas; had not its intolerable smell convinced me that the gas was unaltered, I should not have conceived that it could pass through water undecomposed.

Passed through water undecomposed.

Difference of the author's results with others.

I cannot account for the assertion of Messrs. Gay-Lussac and Thenard and of Mr. Murray, that oximuriatic gas does not, when under the influence of light, exert any action on carbonic oxide: I was inclined at first to suppose, that the difference between their results and mine might be owing to their not having exposed the gasses together to bright sunshine; but I have been obliged to relinquish this idea, since I have found that bright sunshine is not essential, and that the combination is produced in less than twelve hours by the indirect solar rays, light alone being necessary.

The formation of the gas shown.

The formation of the new gas may be very readily witnessed, by making a mixture of dry carbonic oxide and chlorine in a glass tube over mercury: if light be excluded, the chlorine will be absorbed by the mercury, the carbonic oxide alone remaining; but if bright sunshine be immediately admitted when the mixture is first made, a rapid ascension of the mercury will take place, and in less than a minute the colour of the chlorine will be destroyed, and in about ten

minutes

minutes the condensation will have ceased, and the combination of the two gasses will be complete.

Complete absence of water necessary.

It is requisite, that the gasses should be dried for forming this compound; if this p ecaution is neglected, the new gas will be far from pure: it will contain a considerable admixture of the carbonic and muriatic acid gasses, which are produced in consequence of the decomposition of hygrometrical water. Indeed there is considerable difficulty in procuring the new gas tolerably pure; a good a.r pump is required, and perfectly tight stopcocks, and dry gasses, and dry vessels.

Attempt to form it in a hot earthen tube.

I have endeavoured to procure the gas, by passing a mixture of carbonic oxide and chlorine through an earthen-ware tube heated to redness; but without success.

Its specific gravity.

The specific gravity of the gas may be inferred from the specific gravities of its constituent parts jointly with the condensation that takes place at their union. According to Cruickshank, 100 cubic inches of carbonic oxide weigh 29·6 grains; and according to Sir Humphry Davy, 100 of chlorine are equal to 76·37 grains: hence as equal volumes of these gasses combine, and become so condensed as to occupy only half the space they before filled, it follows that 100 cubic inches of the new compound gas are equal to 105·97 grains. Thus this gas exceeds most others as much in its density as it does in its saturating power.

Affinities of chlorine for hidrogen and carbonic oxide equal.

To ascertain whether chlorine has a stronger affinity for hidrogen than for carbonic oxide, I exposed a mixture of the three gasses in equal volumes to light. Both the new compound and muriatic acid gas were formed, and the affinities were so nicely balanced, that the chlorine was nearly equally divided between them. And that the attraction of chlorine for both these gasses is nearly the same, appears to be confirmed by muriatic acid not being decomposed by carbonic oxide, or the new gas by hidrogen.

Name for the new compound.

The chlorine and carbonic oxide are, it is evident from these last facts, united by strong attractions; and as the properties of the substance as a peculiar compound are well characterized, it will be neccssary to designate it by some simple name. I venture to propose that of phosgene, or phosgene gas; from φως, light, and γινομαι, to produce, which signifies

nifies formed by light; and as yet no other mode of producing it has been discovered.

I have exposed mixtures consisting of different proportions of chlorine and carbonic acid to light, but have obtained no new compound.

Oximuriatic and carbonic acid gasses will not combine.

The proportions in which bodies combine appear to be determined by fixed laws, which are exemplified in a variety of instances, and particularly in the present compound. Oxigen combines with twice its volume of hidrogen and twice its volume of carbonic oxide to form water and carbonic acid, and with half its volume of chlorine to form euchlorine; and chlorine reciprocally requires its own volume of hidrogen and its own volume of carbonic oxide to form muriatic acid and the new gas.

Relative proportions of compounds.

This relation of proportions is one of the most beautiful parts of chemical philosophy, and that which promises fairest, when prosecuted, of raising chemistry to the state and certainty of a mathematical science.

II.

A Narrative of the Eruption of a Volcano in the Sea off the Island of St. Michael. By S. TILLARD, *Esq. Captain in the Royal Navy. Communicated by the Right Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S.**

APPROACHING the island of St. Michael's, on Sunday the 12th of June, 1811, in his majesty's sloop Sabrina, under my command, we occasionally observed, rising in the horizon, two or three columns of smoke, such as would have been occasioned by an action between two ships, to which cause we universally attributed its origin. This opinion was, however, in a very short time changed, from the smoke increasing and ascending in much larger bodies than could possibly have been produced by such an event; and having heard an account prior to our sailing from Lisbon, that in the preceding January or February a volcano had

Smoke seen ascending from the sea,

arising from a volcano.

* Philos. Trans. for 1812, p. 152.

burst

burst out within the sea near St. Michael's, we immediately concluded, that the smoke we saw proceeded from this cause, and on our anchoring the next morning in the road of Ponta del Gada, we found this conjecture correct as to the cause, but not to the time; the eruption of January having totally subsided, and the present one having only burst forth two days prior to our approach, and about three miles distant from the one before alluded to.

Two at three miles distance.

Visit to the place.

Desirous of examining as minutely as possible a contention so extraordinary between two such powerful elements, I set off from the city of Ponta del Gada on the morning of the 14th, in company with Mr. Read, the consul general of the Azores, and two other gentlemen. After riding about twenty miles across the NW. end of the island of St. Michael's, we came to the edge of a cliff, whence the volcano burst suddenly upon our view in the most terrific and awful grandeur. It was only a short mile from the base of the cliff, which was nearly perpendicular, and formed the margin of the sea; this cliff being as nearly as I could judge from three to four hundred feet high. To give you an adequate idea of the scene by description is far beyond my powers; but for your satisfaction I shall attempt it.

The volcano described.

Imagine an immense body of smoke rising from the sea, the surface of which was marked by the silvery ripling of the waves, occasioned by the light and steady breezes incidental to those climates in summer. In a quiescent state, it had the appearance of a circular cloud revolving on the water like a horizontal wheel, in various and irregular involutions, expanding itself gradually on the lee side; when suddenly a column of the blackest cinders, ashes, and stones would shoot up in form of a spire at an angle of from ten to twenty degrees from a perpendicular line, the angle of inclination being universally to windward: this was rapidly succeeded by a second, third, and fourth, each acquiring greater velocity, and overtopping the other till they had attained an altitude as much above the level of our eye, as the sea was below it.

As the impetus with which the columns were severally propelled diminished, and their ascending motion had nearly ceased, they broke into various branches resembling a groupe of pines, these again forming themselves into festoons of white

feathery

feathery smoke in the most fanciful manner imaginable, intermixed with the finest particles of falling ashes, which at one time assumed the appearance of innumerable plumes of black and white ostrich feathers surmounting each other; at another, that of the light wavy branches of a weeping willow.

During these bursts, the most vivid flashes of lightning continually issued from the densest part of the volcano; and the cloud of smoke now ascending to an altitude much above the highest point to which the ashes were projected, rolled off in large masses of fleecy clouds, gradually expanding themselves before the wind in a direction nearly horizontal, and drawing up to them a quantity of waterspouts, which formed a most beautiful and striking addition to the general appearance of the scene.

Water spouts drawn up by the clouds.

That part of the sea, where the volcano was situate, was upwards of thirty fathoms deep, and at the time of our viewing it the volcano was only four days old. Soon after our arrival on the cliff, a peasant observed he could discern a peak above the water: we looked, but could not see it; however, in less than half an hour it was plainly visible, and before we quitted the place, which was about three hours from the time of our arrival, a complete crater was formed above the water, not less than twenty feet high on the side where the greatest quantity of ashes fell; the diameter of the crater being apparently about four or five hundred feet.

Rising of the crater above the water.

The great eruptions were generally attended with a noise like the continued firing of cannon and musquetry intermixed, as also with slight shocks of earthquakes, several of which having been felt by my companions, but none by myself, I had become half sceptical, and thought their opinion arose merely from the force of imagination; but while we were sitting within five or six yards of the edge of the cliff, partaking of a slight repast which had been brought with us, and were all busily engaged, one of the most magnificent bursts took place which we had yet witnessed, accompanied by a very severe shock of an earthquake. The instantaneous and involuntary movement of each was to spring upon his feet, and I said "this admits of no doubt." The words had scarce passed my lips, before we observed a large portion of

Eruptions attended with earthquakes.

Fall of part of the cliff.

of the face of the cliff, about fifty yards on our left, falling, which it did with a violent crash. So soon as our first consternation had a little subsided, we removed about ten or a dozen yards farther from the edge of the cliff, and finished our dinner.

Farther account.

On the succeeding day, June 15th, having the consul and some other friends on board, I weighed, and proceeded with the ship towards the volcano, with the intention of witnessing a night view; but in this expectation we were greatly disappointed, from the wind freshening and the weather becoming thick and hazy, and also from the volcano itself being clearly more quiescent than it was the preceding day. It seldom emitted any lightning, but occasionally as much flame as may be seen to issue from the top of a glass-house, or foundery chimney.

On passing directly under the great cloud of smoke, about three or four miles distant from the volcano, the decks of the ship were covered with fine black ashes, which fell intermixed with small rain. We returned the next morning, and late on the evening of the same day, I took my leave of St. Michael's to complete my cruize.

On opening the volcano clear of the NW. part of the island, after dark on the 16th, we witnessed one or two eruptions that, had the ship been near enough, would have been awfully grand. It appeared one contiuued blaze of lightning; but the distance which it was at from the ship, upwards of twenty miles, prevented our seeing it with effect.

The volcano quiet, and 80 yards above water.

Returning again towards St. Michael's on the 4th of July, I was obliged, by the state of the wind, to pass with the ship very close to the island, which was now completely formed by the volcano, being nearly the height of Matlock High Tor, about eighty yards above the sea. At this time it was perfectly tranquil, which circumstance determined me to land, and explore it more narrowly.

Landing on the island.

I left the ship in one of the boats, accompanied by some of the officers. As we approached, we perceived it was still smoking in many parts, and upon our reaching the island found the surf on the beach very high. Rowing round to the lee side, with some little difficulty, by the aid of an oar, as a pole, I jumped on shore, and was followed by the other officers

officers. We found a narrow beach of black ashes, from which the side of the island rose in general too steep to admit of our ascending; and where we could have clambered up, the mass of matter was much too hot to allow our proceeding more than a few yards in the ascent.

The declivity below the surface of the sea was equally steep, having seven fathoms water, scarce the boat's length from the shore, and at the distance of twenty or thirty yards we sounded twenty-five fathoms.

Less than a mile round.

From walking round it, in about twelve minutes, I should judge that it was something less than a mile in circumference; but the most extraordinary part was the crater, the mouth of which, on the side facing St. Michael's, was nearly level with the sea. It was filled with water, at that time boiling, and was emptying itself into the sea, by a small stream about six yards over, and by which I should suppose it was continually filled again at high water. This stream, close to the edge of the sea, was so hot, as only to admit the finger to be dipped suddenly in, and taken out again immediately.

The crater full of boiling water.

A peninsula joining the main island.

It appeared evident, by the formation of this part of the island, that the sea had, during the eruptions, broke into the crater in two places, as the east side of the small stream was bounded by a precipice, a cliff between twenty and thirty feet high forming a peninsula of about the same dimensions in width, and from fifty to sixty feet long, connected with the other part of the island by a narrow ridge of cinders and lava, as an isthmus of from forty to fifty feet in length, from which the crater rose in the form of an amphitheatre.

Ascent of the isthmus.

This cliff, at two or three miles distance from the island, had the appearance of a work of art resembling a small fort or block house. The top of this we were determined, if possible, to attain; but the difficulty we had to encounter in doing so was considerable; the only way to attempt it was up the side of the isthmus, which was so steep, that the only mode by which we could effect it, was by fixing the end of an oar at the base, with the assistance of which we forced ourselves up in nearly a backward direction.

Having reached the summit of the isthmus, we found another difficulty, for it was impossible to walk upon it, as the descent on the other side was immediate, and as steep as the

one

one we had ascended; but by throwing our legs across it, as would be done on the ridge of a house, and moving ourselves forward by our hands, we at length reached that part of it where it gradually widened itself and formed the summit of the cliff, which we found to have a perfectly flat surface, of the dimensions before stated.

Flag planted, and the island named Sabrina.

Judging this to be the most conspicuous situation, we here planted the Union, and left a bottle sealed up containing a small account of the origin of the island, and of our having landed upon it, and naming it Sabrina Island.

Fishes destroyed by the eruption.

Within the crater I found the complete skeleton of a guard-fish, the bones of which, being perfectly burnt, fell to pieces upon attempting to take them up; and by the account of the inhabitants on the coast of St. Michael's, great numbers of fish had been destroyed during the early part of the eruption, as large quantities, probably suffocated or poisoned, were occasionally found drifted into the small inlets or bays.

Nature of the island.

The island, like other volcanic productions, is composed principally of porous substances, and generally burnt to complete cinders, with occasional masses of a stone, which I should suppose to be a mixture of iron and lime-stone; but have sent you specimens to enable you to form a better judgment than you possibly can by any description of mine.

III.

New Method of making Bricks, so as to form cheaper and firmer Buildings, and useful underground Drains: by JOHN STEPHENS, *Esq. of Reading, in Berkshire**.

SIR,

Bricks divided nearly through,

I HAVE sent, for the inspection of the Society of Arts, &c. three closure bricks, which on examination you will find to have been cut three fourths of the way through in the middle

* Trans. of the Soc. of Arts, vol. XXIX, p. 39. The silver medal was voted to Mr. Stephens.

by

by a wire, and the whole of the way through at each end, which leaves the ends square and handsome for work.

The bricklayer, to divide each brick in length, has only to take the brick in his left hand, with the mark, or cut, downwards longitudinally; and by one smart blow with the trowel he will have two complete king-closures, with which he can easily make four common closures.

easily cut for closures.

I have shown them to many workmen, who all approve of them. I had two hundred and fifty of them made by a brick-maker for an experiment, and I have ordered two thousand more. The builders who do the principal part of my work have had some on their own account, and have since increased their orders. I have no doubt when they are better known they will come into general use.

A considerable saving in labour and waste of bricks may be effected by their use, particularly in walls where piers are built, and where there are many openings; the work will also be rendered more substantial. There will be a saving in room and materials where the back of a chimney is built against a straight wall, particularly in flues for low buildings. They will be found useful in cities or large towns by being placed in partition walls instead of lath and plaster, and be a check to the ravages of fire. They will be useful in preventing the passage of rats and mice, and the disagreeable smell occasioned when they die betwixt lath and plaster or wainscot. They will also answer for draining of land, and will form cheaper small drains from houses than any other method. They may be cut in other forms or directions for particular purposes according to the uses for which they are intended.

Their advantages.

The additional expense of dividing them by the wire is about two shillings per thousand, it is generally done after they have been moulded one or two days according to the dryness of the season.

Additional expense of making.

I flatter myself, that, if this communication meets with the approbation of the Society, it will render a benefit to the public.

I am, Sir, with much esteem,

Your most obedient servant,

JOHN STEPHENS.

Reading, October 31, 1810.

DEAR

DEAR SIR,

Saving in their use.

On inquiry from builders, I am informed, that the saving by the use of the bricks I have invented will be from two and a half to nearly five per cent, in a five-window house in brick work and labour, in a front of forty feet with or without piers. In ornamental brick piers for gateways, I think the saving of bricks by means of cutting may be very considerable, and in the labour still more, beside the work being done more sound and substantial.

11 inch wall.

I am using a few of them in an eleven inch brick wall, (a system hitherto entirely new), in a westernly aspect, as a preventive or guard against the effects of weather, and it will, in point of dryness, be equal to a fourteen inch wall. I have enclosed a letter from Benjamin Garroway, a bricklayer, who has requested me to let him have all the bricks I have of this kind, and to bespeak more for him. I have also sent a certificate from Mr. Robert Wright, who is extensively engaged in buildings.

Drains.

The drains for agricultural purposes might be done by women or children, except the digging of the drains, especially two inch drains. With respect to longer drains, if they are required of four inches, and to be covered with brick, I would recommend the bricks to be laid anglewise, in order to promote strength in covering.

Drawback of duty.

It would be of great importance if parliament would allow a drawback of the duty on all bricks employed in draining.

Mode of making the brick.

Every brick, intended for the operation I recommend, is taken off the stack two or three days after it is moulded. It is then put on a stool or board, and a wire, about the size of No. 23, is pressed on the upper side of the brick, so as to pass through each end of it; it is then immediately placed on the stack again, and afterward burned.

I am, dear sir,

Your most obedient humble servant,

Reading, December 8, 1810. JOHN STEPHENS.

Letter from Mr. RICHARD BILLING, to *Mr.* STEPHENS.

SIR,

Remarks on the utility of these bricks.

Agreeably to your request, I have taken into consideration the utility of your closure bricks, and beg leave to say, that

that my opinion coincides with yours, as to their advantage, in new chimnies, which are intended to be built against old walls. In constructing a new chimney it is generally considered absolutely necessary, that the same should be worked up close to the old wall, but completely unconnected, in order that it might settle from the old; in this case, it is very desirable to make the back of the chimney as thin as possible, that it may project as little as convenient; and in building piers, particularly small ones, either for gate-ways or fronts of houses, where there are many bricks, and in the present mode, which is so frequently adopted, of two inch recesses at the exterior of the windows, your closures would be much preferable even in appearance to a brick which has been cut with a trowel, with the surface of course defaced.

Closure bricks might be adopted as a cheap and useful drain by a common brick flat, with two closures laid on the same two inches asunder, or four inches, and reversed. Drains.

Your closures would be useful in all kinds of ornamental brick work. Ornamental work.

Two inches is a very desirable brick, but most times avoided in consequence of the waste in cutting common bricks, and difficulty in producing a smooth face, which would be completely obviated by the introduction of closure bricks. Two inch bricks desirable.

I remain, sir,

Your obedient humble Servant.

Reading, RICHARD BILLING.

December 3, 1810.

Letter from Mr. BENJAMIN GARROWAY, to *Mr.* STEPHENS.

SIR,

I am of opinion, that, if closures were made for general use, two and a half per cent would be saved in brickwork of small piers, flues of chimneys, or where there are any bricks in ornamental works. Common bricks frequently will not cut more than one closure; and if your bricks were to be always had, they would be much more useful. Farther remarks.

I remain, sir,

Your humble servant,

Ruscombe, BENJAMIN GARROWAY.

December 5, 1818.

Letter

Letter from Mr. ROBERT WRIGHT *to Mr.* STEPHENS.

SIR,

Having examined your method of cutting bricks, I am of opinion, that they would be particularly useful in all kinds of brick work, make a considerable saving in labour and materials, and that a much superior bond would be obtained by your improvement.

You are perfectly at liberty to make any use you please of my opinion.

I am, sir,

Your very obedient servant,

ROBERT WRIGHT.

No. 5, New North Street, Red Lion Square, London, December 6, 1810.

Description of the drawings of Mr. Stephens's method of cutting bricks for various purposes. See Plate VI, fig. 2—7.

Explanation of the plate.

Fig. 2, of plate VI, is a plan of the upper surface of a common brick: the line *a a* is a cleft cut nearly through the brick while it is soft by means of a piece of wire, as is shown in the section, fig. 3; where the section of brick is shown at BB, placed on the wooden block A, a piece of wire *b b* with a loop at each end is pressed down into it, so as to divide it into two parts, except the part C, which the wire will not cut through because of the curvature it acquires in being pressed into the brick. A brick of this kind, being burnt, may be broken in two halves by one cleft with the trowel, which will be found very useful in many cases which constantly occur in brickwork, and will be far superior to the present mode of hacking the bricks, both for the soundness and appearance of the work, and will be done in less time.

Figs. 4 and 5 show the application of these divided bricks to draining, where AB are the ends of the two halves of a brick, and C D tiles, forming the top and bottom of the drain, this method forms a square drain.

Fig. 5 shows how a triangular drain may be made with half the number of bricks of the foregoing, that is one half brick A, and two tiles C D.

Fig.

Fig. 6, is a plan of a brick divided diagonally, and fig. 7 shows how these halves may be disposed to form a triangular drain: the letters show the same parts in each of these two figures: the bottom, D, may be made of tile, or of a brick cut in half in its thickness: the scale annexed to the figures will show the dimensions of the different drains.

IV.

A temporary Rick, to secure Corn in Sheaves in the Fields till quite dry; also Clover, Pease, and Beans: by WILLIAM JONES, *Esq. of Foxdown Hill, near Wellington, Somersetshire*.*

SIR,

THE very unusual quantity of rain, that fell during the months of August and September last, with scarcely two days of dry weather following, in this neighbourhood, put farmers to the necessity of having recourse to various modes of preserving their corn; and, as I understand the Society of Arts has offered a gold medal for the cheapest and best mode of harvesting corn, and also for making hay in wet weather, superior to any hitherto practised, I beg leave to communicate some experiments I made last summer, and the result of them. In the first place, I put some wheat in small round ricks, or wind-rows, made in the common way of this county; but afterward recollected, that the uncommon wetness of the ground might render the under part damp. I thought it prudent to examine them, (about ten days after they were set up), and found my apprehensions so well founded, that I had the whole spread abroad; and have no doubt, that, if they had remained a little longer, the corn would have been materially injured; not the bottom only, for it had contracted dampness a great way up the ricks, insomuch that I turned my attention to devise some better mode of preserving my barley in case the weather continued so rainy, as it afterward proved. I had observed in some wet

Harvesting in wet weather.

Small ricks of wheat

injured by the dampness of the ground.

* Trans. of the Soc. of Arts. vol. XXIX, p. 46. The silver medal was voted to Mr. Jones for his invention.

Barley the same,

seasons before this, that many of our farmers, not being able to get their barley dry enough to put into a large rick, had set up narrow ricks, containing the produce of an acre or two, each in different parts of the field where it was grown, for the sake of expedition; and though some straw was put under them, yet the bottom contracted a great degree of dampness, so as to occasion it to smell old, and the clover was killed where these ricks had stood.

and the clover killed underneath.

Method of obviating these inconveniences.

My object was to prevent both these injuries; and it occurred to me, that four gate hurdles would answer both purposes, by setting the two outside ones perpendicular, and two middle ones inclining against and supporting each other.

Stand for the rick.

These hurdles are usually eight feet long; the two heads, in which the four bars are mortised, have pointed heads of about a foot and a half long; the two outside ones are to be forced into the ground nearly their full length, so that the middle brace may rest on the ground to afford some support; and the two middle ones about six inches, to keep them steady. The foot of the second hurdle should be set two feet from the foot of the first, the third three feet from the second, and the fourth two feet from the third, making seven feet, and occupying a space of seven feet by eight, for barley or oats; but wheat, being longer in the straw, requires the distance to be wider, viz. three feet from the first to the second, three feet from the second to the third, and three feet from the third to the fourth, which will be nine feet by eight.

It will be proper to put seven or eight small stakes, (a little bigger than a man's thumb), from the second bar of the first hurdle to the second bar of the second hurdle, and from the second bar of the third to the second bar of the fourth, to support the sheaves from the ground, to admit air under and prevent injury to the growing clover; or small poles may be used extending from one outside hurdle to the other. The appearance of the ends of the hurdles will be as in the engraved plan, Plate VI, fig. 1, and section Plate VII, fig. 1, which show where the small stakes are to be placed to prevent the sheaves touching the ground, for there will be but a slight pressure on them, since the ground ends of the sheaves are to be put against the hurdles A B, and the

Structure of the rick.

ears

ears of the corn a little elevated to rest against the hurdles C D: so that the ears of the corn will be all within side, and have the benefit of the air between C and D. It is to be observed, that the hurdles C D, being but six inches in the ground, and the hurdles A B nearly eighteen inches, the two former will be a little higher than the two latter; which is necessary for two reasons, one is, that the higher these are, the higher the air is admitted to the middle of the rick, and the more they elevate the tops of the sheaves in the middle, for the ground ends should be lowest to shoot off the rain. But as it will be found, that, after two or three rows are placed around the tops of the hurdles, (for the ricks should be circular), the ground ends of the sheaves being largest, the tops will become nearly level; when it will be necessary to put four sheaves as at G G in the middle horizontally, forming a square, open in the centre, which will admit air from the top of the middle hurdles C D, through this space, to the middle of the rick, as the ears of each sheaf are just to meet only in the middle resting on these four sheaves*; which will give such an elevation to the tops of them, that the ground ends will be sufficiently inclining downwards to shoot off any rain that may fall. In forming the roof, the sheaves are of course to be put farther in every time they are put around, till the roof terminates in a point, when two sheaves, with the tops downward spread abroad and bound with a straw band, will secure it from a great deal of rain; but if the corn is to remain out long, a little reed or thatch may soon be put on each rick.

Model.

Fearing I might not have been sufficiently explicit in de- describing this plan, it has occurred to me, that it would be better to send a model, containing 100 sheaves, made to a scale of an inch to a foot, as to the length of the hurdles, the distance from each other, and the size of the sheaves, also to exemplify every particular of it.

Barley saved in this manner,

The weather being so rainy for some days after my barley was cut, with every appearance of more rain, I determined,

* If the corn should be very damp, and the rick made high, four other sheaves may be put higher up to convey a greater circulation of air, and operate as a bond to connect the sheaves in the middle, so that they cannot possibly slide outwards.

on having a few hours intermission of rain, to get the middle of the field, which was a little more dry than the rest, and to put it in small ricks, containing more than the produce of an acre, on these hurdles in the same field; it was in such a damp state as to be totally spoiled in the common rick, but was taken from these ricks into a barn in the month of January last, perfectly dry, the straw much better than could have been expected, the grain good, having been proved to grow well; for having some doubt on account of being put together so damp, I had it first tried by putting a few grains in a cloth into the earth, and have since sown it, and no other this spring, and I never had a better prospect of a good crop. The remaining part of the barley, that was left on the ground, was not taken in till ten days afterwards, the grain much grown, a great deal wasted by frequently turning, and the straw spoiled.

while some other was spoiled.

I flatter myself it will be admitted, that in wet seasons, or when harvest is so late, that, as the days decrease, the dews increase, and of course remain so long that there are but few hours in a day for drying, even if there should be no rain; this method will afford perfect security to corn that is cut dry, and put up in this manner immediately from the sithe or sickle: because, if there should be grass in it, the ground end of every sheaf will be withoutside, exposed to the sun and air to dry; and as for the grain, no part of it can get damp, because the ears but just meet in the middle, through which the air passes from the bottom to the top sufficiently to dry it. I have mentioned sheaves, because in this county barley and oats are generally bound as well as wheat; but both the former may be placed in these ricks without binding, as I had some barley put in one of them (by way of experiment), and think it to be the better mode when there is much grass in it, by carefully keeping the ears together when carried to the hurdles, where a man is ready to put it up to another on the top, and to place the ears inwards; and it is done in as short a time as the like quantity is put on a waggon, with this advantage, that, whereas a waggon with three or four horses goes over the clover to the great injury of it in wet weather, by this method the corn is carried by women or children in their arms to the hurdles, without the

Barley and oats made into sheaves, but may be stacked without it.

No waggon to injure the clover.

the least injury to the clover, a consideration fully adequate to a little extra expense, if any, beside that of being more expeditiously secured; for every practical farmer will be sensible in how short a time an acre of corn may be carried from the circumference of an acre to its centre. As to the time of fixing these hurdles, I have ascertained, that two people can fix them in five minutes, and one rick would contain the produce of two acres of barley or oats. The other advantages, beside the corn being thus sooner secured, are, that no more attendance on it is required, so that a farmer's attention may be better directed to his other harvest concerns, and, that one or two of these ricks at a time, (as may be convenient), may be taken into a barn to thrash, whereas a part of a large rick cannot be taken in without the trouble and expense of thatching the remainder, and being subject to the risk of rain before it may be covered again.

Time of fixing the stand.

Other advantages.

I trust it will be seen, that by this plan there must be a great saving of the quantity as well as the preservation of the quality of the grain, which is known often times to shed a great deal by being frequently turned to get dry. Before I thought of this expedient (last barley harvest), I am clear, that a field of pease of mine required to be turned so often, that more shed out than were sown; and a farmer in this neighbourhood had a good crop of eight acres of vetches reduced to sixty bushels, by so frequently turning them for three weeks, without getting them dry at last; whereas an acre or two might have been taken up in this way a few days after they were cut, and the seed would have got sufficiently hard, but the greater part of these were so soft as to be much bruised in thrashing, and it was to be feared a great part of them would not vegetate. I had an opportunity of knowing the quantity, having the tithe of them; and proving the injury by the loss of my crop in sowing them, insomuch that the land has been since ploughed.

Saving of grain, as well as preserving from injury.

Although I have not tried it, yet I think it is not to be doubted, but that this mode may be applied with equal advantage to clover hay, and clover seed, before it may be dry enough to put into a large rick, by being placed in this situation to dry without being so frequently turned as to deprive the hay of its finest parts, and subject the seed to great

Application to clover for hay or seed

waste.

and to grass cut for hay.

waste. In cases also when meadow hay may be dry enough to put in large cocks on the appearance of rain, how much injury do they receive by the bottom being rendered so wet as to occasion a dampness some way up, and require much time to throw abroad to dry? Whereas, in the same state of dryness, how many of such cocks may be put on four hurdles; and the bottom instead of being wet and injured will be perfectly dry, having air circulating under it, and from the two middle hurdles quite to the top; if a sheaf of reed was to be drawn up through it, as the hay got higher: a bundle of straw on the top would secure it from rain. Or, instead of a reedsheaf drawn up, a couple of small faggots of wood, or three or four poles bound together, and placed horizontally about the middle of the rick, to admit air at each end, and render it dry enough to be carried on to a rick without farther trouble or risk.

Hay injured by exposure to too much sun.

Hay is known to receive injury, not only from rain, but even from fervent sunshine, when nearly dry, if not frequently turned: as may be observed by the change of colour and loss of smell, which many farmers in this neighbourhood experienced in the summer of 1809, for want of hands to turn it sufficiently. I have seen an infusion of such hay made in a tea-pot, and compared with an infusion of the like quantity of good hay in another: the former was very deficient both in colour and taste to the latter, and the quality of it, of course, much deteriorated.

The straw improved.

We know that straw, particularly of barley or oats, will be much injured by being long on the ground exposed to soaking dews, and perhaps to alternate rain and sunshine; and may it not, when protected from them by this mode, be far superior for cattle to what we are at present aware of? Beside the advantages of grain, hay, and straw, being thus better preserved, and less expense of labour than by repeatedly turning in rainy seasons, there is another advantage of no small consequence, that the crops may be removed, and put on hurdles in another field, (without any hindrance to sheep feeding therein) when the land whence they were taken may be immediately ploughed; for instance, after pease, to facilitate a better fallow, (than if delayed), to be succeeded by wheat, and ploughing clover lays for wheat,

Farther advantage.

and

and also preparing land for turnips after vetches, to accelerate the sowing; in which case, the delay of a few days has frequently occasioned a total loss of the crop.

Cheapness and expedition.

It is an essential consideration, that the expense attending improvements should not counterbalance their utility: and I flatter myself, there can be no objection to this mode on that score, because gate hurdles are useful appendages to a farm, in any county, for other purposes, when not used on this occasion; and in this and other counties they are requisite for dividing turnips for sheep; and, as to expedition, which is of great importance in harvest concerns, four of these hurdles (as I have already observed) may be fixed in five minutes.

If, therefore, the Society for the Encouragement of Arts, Manufactures, and Commerce, instituted for the laudable purposes which it professes, should think my plan combines utility with cheapness and expedition, I should consider myself flattered by their approbation; and feel a degree of satisfaction in the reflection, that I have not turned my thoughts in vain to a subject, which must be allowed to be of great importance. I am, sir,

Your most obedient servant,

Foxdown Hill, June 7th, 1810. W. JONES.

SIR,

I have been favoured with your letter, acknowledging your having received my model of a temporary rick, and recommending me to send certificates of its use.

State of the barley saved.

I have to add, that the barley I had put on these hurdles last year was done in my presence, by the same man who removed it afterward to the barn, thrashed, and sowed it; he is ready to attest my former statement of the hurdles requiring only five minutes time to fix in the ground; of the barley preserved by them growing perfectly well, with a prospect, from its present appearance, of yielding a good crop; and with this farther remark, that it was so damp when put upon the hurdles, that he was apprehensive it would be spoiled, and was much surprised when he took it into the barn, to find it so perfectly dry.

Temporary hovel for sheep.

I notice your query, whether these hurdles could not be applied to the purpose of temporary hovels for sheep, in

wet

wet weather? I think, that if two of them were fixed eight feet apart, and two others placed on the top of them, covered with straw, reed, rushes, heath, or furze, they would form a covered hovel of eight feet square, and afford great protection to sheep in wet weather, (particularly just after being shorn;) and to ewes in the lambing season also, if some, that were the most forward with lamb, were selected and put into enclosures, where one end of each hurdle might be put against a hedge, or against a wall, or end of a hovel. These hurdles, covered in like manner also, would be useful, if a number of them, proportioned to the quantity of sheep, were put in the form of a square, in any part of a field, in hot weather, to afford shade. They would induce the sheep to lie there, and answer the purpose of folding, as they could easily be moved to such part of the field as wanted improvement; and the sheep would be more at ease than when creeping under hedges, to the no small detriment of their wool.

Farther use of the temporary stand.

I have to report to the Society, that I have this harvest made use of the hurdles on a larger scale, viz. to keep raking wheat separate from the sheaf, and which was too damp to put in sheaf; and also in small ricks of wheat for seed, to save the trouble of taking it from a larger rick, before the whole was wanted to be thrashed; and for my tithe wheat, that was not sufficiently dry to put into a barn.

Late crop of pease.

I had also five acres of white pease, which were drilled where a crop of vetches had failed, so late as the 12th of May; they proved to be a very great crop, but they ripened so late, and the tops of the haulm were so green, from having shot out to an extraordinary length, that they were not all carried till the 27th of last month. At one time I almost despaired of ever getting them dry, owing to the heavy dews which fell during the night, and continued during most of the day, so as to afford but a few hours to dry my crop. I therefore took up six waggon loads from the middle of the field, on the 25th of last month, and put them on twelve gate hurdles adjoining each other, for the purpose of making one roof, and set the hurdles in the manner of my ricks. The first two loads were put on four of these hurdles at one end, which would contain four loads

if

if necessary; the next two on the adjoining four hurdles; and the other two loads on the four remaining hurdles; so that though these three ricks were close to each other, yet being set up separately, they admitted air between each, from the bottom to the top, and yet adjoined sufficiently to make one continued roof to be thatched together.

When these six loads were removed from the field, I had room to turn the remaining parcels towards each other, and more towards the middle of the field, so as to have more air to dry. But they were not sufficiently dry till the 27th, when they were carried to another set of sixteen hurdles ready to take them, and each waggon load laid over the whole length of 16 hurdles, not being so damp as to require being carried up in separate ricks, as the former six loads. Some of these pease have been already thrashed, and prove to be in very good condition, as also the haulm, which is perfectly dry and sweet for cattle.

One of these ricks of pease, and probably some of the ricks of wheat, will not be taken in till the month of February next: they may therefore be inspected by any member of the Society, who may visit this neighbourhood.

I have enclosed a certificate from Mr. Waldron, a gentleman of this parish, who farms his own estate; and another certificate from Mr. Hewitt, also of this parish, who is esteemed a respectable and intelligent farmer; he rents a farm from Mr. Ware, brother to Mr. Ware of the house of Ware, Bruce, and Co. London.

I am, sir,
Your most obedient servant,
W. JONES.

Foxdown Hill, Oct. 30*th*, 1810.

SIR,

Size of temporary corn rick.

Agreeably to your request, I lose no time to give you the information you desire, respecting the temporary corn ricks, and the size they may be made. The space between the two outside hurdles contains about sixty sheaves on each side, or one hundred and twenty in the whole, to reach the top of the hurdles. Every round of sheaves afterward takes forty sheaves or upwards, say fifteen rounds high, which makes six hundred sheaves, and which will raise the rick

rick about eight feet from the tops of the hurdles. It will require about seventy sheaves from the top of the above fifteen rounds, to the top of the conic roof. Four sheaves crossing each other, five times in the centre of the rick, will form in the whole twenty, making as follows:

120	Sheaves to the top of the hurdles.
600	Sheaves from the tops of the hurdles, to the commencement of the roof.
72	Sheaves in the conical roofs.
20	Sheaves in the cross or bonds of the rick.
812	Sheaves, or upwards of 81 shocks in each rick, which is more than the average produce of an acre.

Length of sheaves.

The wheat in this part of the country is reaped near to the ground, and my sheaves, this year, are about $4\frac{1}{2}$ feet long, for which the distance of 3 feet $2\frac{1}{2}$ inches between the outer hurdles, and 3 feet between the inner hurdles, is calculated. The distance should be regulated by the length of the sheaves of barley and oats. When shorter than four feet, the rick should be oblong instead of round.

Faggots useful at intervals.

Faggots of wood, placed at intervals within the rick, will be found particularly useful, where pease, vetches, clover, hay seeds, and meadow hay, are put into these ricks, as the faggots will promote a greater circulation of air.

The number of the cross sheaves should be according to the dampness or dryness of the corn, either in every row, or every second or third row.

Reference to Plate VII, *fig.* 1, *the Section of Mr. Jones's Temporary Corn Rick.*

The letters, describing the same parts of the construction of the rick, agree with those in Plate VI.

A B, the two upright outside hurdles.

C D, the two inclined hurdles.

E E, the poles or sticks on which the sheaves are to be first placed on commencing the rick, and which cross the hurdles.

H H H H, the sheaves composing the body of the stack.

I I,

I I, the conical roof, the lower part of which projects sufficiently over the body of the rick, to cover it from wet, and in this roof, each round of sheaves is to be placed so as to cover the ears of the sheaves below, and gradually rise to nearly a point, over which a bundle, containing two or three sheaves, with the butt ends upwards, and tied together, cover the centre or uppermost point of the rick.

V.

Improvement in the Acorn Dibble; by Mr. CHARLES WAISTELL, *of High Holborn.**

SIR,

IN consequence of information, that Government wanted intelligence respecting the best mode of dibbling acorns, I have made an improvement on the acorn dibble in the Society's repository, which I presume will answer well the desired purpose. I therefore send herewith a drawing of it, requesting you will have the goodness to lay it before the Society of Arts &c.

Dibble for acorns.

Thorn bushes and thickets are the natural guardians of young oaks from the depredations of cattle of all kinds, on forests and other grounds on which they pasture. By means of this implement, acorns may be deposited in the interior of bushes, as well as in open grounds, with rapidity and accuracy. And presuming that such an implement would be of great utility to many individuals, and also to Government, I wish much to have it made known as generally as possible among those who are most likely to profit by it; and which I think may be best effected by the Society of Arts, &c. giving an engraving of it in their next volume; provided they concur with me in thinking it may be the means of rearing an increased number of oaks, to promote which every possible facility should be given.

Bushes and thickets the natural guardians of young oaks.

Permit me on this occasion to observe, that many proprietors of landed property are not sufficiently aware, that a greater or less proportion of almost every estate would, if judiciously

Planting on estates not sufficiently attended to.

* Trans. of the Soc. of Arts, vol. XXIX, p. 60.

diciously planted, pay the proprietor much more than the rent it could be let for to a farmer. It would, therefore, give me great pleasure to see in the Society's volumes more communications from successful planters. I trust there are numerous persons of this description, who want only to be reminded, how greatly they might benefit individuals, as well as their country, by publishing or communicating to you such well ascertained facts of their success as planters, as they may be in possession of; and in order to direct their attention to the nature of the information that is chiefly wanted, I beg leave to refer them to pages 80 and 81 of the Society's 27th volume*, wherein numerous particulars respecting the planting, management, and produce of woods, are enumerated.

I am, Sir,

Your obedient servant,

CHARLES WAISTELL.

No. 99, *High Holborn,*
June 12, 1811.

Reference to the Engravings and Section of Mr. Waistell's Improvement of the Dibble for Planting Acorns. Plate VII, *figs.* 2, 3 *and* 4.

The dibble described.

a represents the handle of the dibble, which dibble is a rod $\frac{3}{4}$ of an inch in diameter, movable in the tube of a stave, which stave is externally about two inches diameter; *b* a tin or metal tube fixed on the exterior part of the stave, and of the same bore or aperture as the tube of the stave. When a hole is made in the earth by the point of the dibble *d*, the acorn is dropped down the metal tube, and on drawing up the dibble by its handle to the height of the letter *e*, the acorn *c* passes through a large opening into the dibble tube, and thence falls into the hole made by the point of the dibble in the earth; when by moving backwards and forwards the cross handles *gg*, fixed on the top of the hollow stave, the soil surrounding the hole in the earth is loosened by the iron wings *ff*, and deposited on the acorn. Fig. 4, *h*, shows a section of the iron wings *f f* belonging to the bottom of the hollow stave.

Method of using it.

Supposing that you wish to plant an acorn in the middle of any bush, you are to press the instrument through it into

* See Journal, vol. XXVII, p. 307.

the

the ground, make a hole in the earth by the point of the dibble rod, then raise the rod above the hole where the two tubes communicate, drop the acorn down the tube *b*, which falls immediately through it and the lower part of the stave tube into the hole previously made by the rod, which hole is instantly covered by the soil raised by the wings. The dibble rod may be occasionally passed down the metal tube, to be certain of its being perfectly clear.

VI.

On the apparent Streaks of Light, left sometimes by falling or shooting Stars; and on their apparent rectilinear Courses in the Atmosphere. In a Letter from JOHN FAREY, *Sen. Esq.*

To W. NICHOLSON, Esq.

SIR,

SOME months ago I was induced, by the frequent references to shooting or falling stars, as being a phenomenon in and connected with particular states of our atmosphere, that I had noticed in the improved Meteorological Journals for some time previously, inserted in yours and other periodical works, to address the gentlemen engaged in these observation, through your means (see Vol. XXX, p. 285), to suggest, from considerable series of observations by myself and others on these bodies, that their appearance at particular times was in no way influenced by the particular state of our atmosphere, (any more than the appearance of the moon at particular periods of her revolution); except only, by the absence of clouds and haziness to obscure, and of greater degrees of light from other sources to overpower them (as the stars &c. are by the day-light); and to request the minute attention of these and other meteorologists to the particular circumstances, decisive of these my suggestions being well or ill founded. Since which, and probably in consequence, the references to these phenomena, before so frequent, have nearly or altogether ceased in the Journals of Meteorological observations referred to; but nothing farther has appeared on the subject, until your last number, in which, p. 229, a very respectable and veteran meteorologist,

Shooting stars not connected with the state of the atmosphere.

Mr. De Luc's hypothesis,

meteorologist, J. A De Luc, Esq., has spoken of this phenomenon as being occasioned by jets or streams of some fluids, from the surface of the Earth into the atmosphere, and the falling again of the same in a phosphorescent state, &c.

does not agree with the facts.

The train of light an optic illusion.

The same sincere desire for the extension of our knowledge on this very interesting subject, that induced me formerly to address the gentlemen alluded to, now prompts me to state, that the above explanation of the phenomenon by Mr. De Luc is at variance with all the best observations that I have made, or seen recorded, concerning these meteors: and that the fact of their general approach to rectilinear courses* exactly accords with all their other appearances, as being those of bodies describing parts of very large ellipses, in one of the foci of which the centre of our planet is situate; and all, except a very few of them (which from their apparent size would not be called falling stars by any one, but large meteors, as I apprehend), are moving so distant from us, and in such rare parts of our atmosphere, though with satellitic velocity, as not ever to be turned suddenly out of their courses, by highly condensed air before them, as Mr. De Luc with great appearance of probability maintains the electric spark to be deflected on or near to the surface of the Earth. And farther, I beg to repeat my conviction, that all the streaks of light, that I have sometimes seen, as following shooting stars and meteors, are to be referred to the eye remaining stationary, or nearly so, during the observations that were followed by streaks of light: and I venture to recommend, as decisive of this question, that two, three, or more intelligent persons should observe in conjunction, and each one without communication with the others, write down as quickly as possible the circumstances attending his observation: and if then it happens, as it invariably has done with myself and others, either that there are no streaks seen, or that some will see streaks and others none, according as their eye does or does not adapt itself to the apparent motion of the meteor; then this supposed evidence of streams of phosphorescent fluids

* But which is not their invariable appearance, as I have sometimes seen them move in curves.

must

must be abandoned, as untenable. Hoping that the subject will ere long attract the attention of observers in earnest, and be fully elucidated,

I remain, sir,

Your obedient humble servant,

J. FAREY.

Westminster, July 2, 1812.

VII.

On Galvanic phenomena. In a Letter from J. A DE LUC, ESQ, F. R. S.

To W. NICHOLSON, Esq.

SIR,

DR MAYCOCK's papers in your Journal have much interested me, as affording the opportunity of very useful disquisitions on important objects of natural philosophy; they are contained in your Numbers 131 and 144, the former of which will be the subject of my present remarks. Dr. Maycock's observations

This paper has the following title: *Observations on the hypothesis, which refers chemical affinities to the electrical energies of the particles of metals*: an hypothesis introduced by Sir H. Davy in a lecture to the Royal Society in 1807. The paper of Dr. Maycock (as mentioned in a note) had before (deservedly) obtained the gold medal of the Medical Society of Edinburgh, on this question: "Whether are the "phenomena produced in the decomposition of bodies by "*galvanism* capable of being explained by the usual princi"ples of *chemical attraction*; or do they seem to establish "the theory, that *chemical phenomena* depend entirely on "the *electrical energies* of the particles of matter." on the reference of chemical affinity to electrical energy.

I had already, Sir, refuted this last hypothesis almost a year before, in your Journal for June 1810, by the analysis of the phenomena of the *galvanic pile*; but Dr. Maycock has more deeply treated this subject in the first two *sections* of his paper, proving irresistibly, by direct chemical experiments, that they could not be referred to *electrical energies*. And indeed Sir H. Davy himself has since expressed some doubts on his own theory. Complete refutations of the hypothesis.

But

Objection to Dr Maycock's 3d section.

But after having acquiesced with pleasure in the merit of these first two *sections* of Dr. Maycock's paper, I cannot do the same with regard to the third *section*, nor can I acquiesce in the consequences he derives from it in your No. 144. But how can it be that Dr. Maycock, though he addresses to you his papers, never mentions five of mine on the same subject, inserted in your Nos. of June, August, October, and December 1810, and January 1811, in which I have treated, from experiments, all the parts of his system? Whatever be the cause of this singular circumstance, the following discussion of the subject comparatively to his system, which I hope he will see, will certainly contribute to throw light on the most important points of *electricity* and *galvanism*.

He supposes the different electrical states of metals to be produced at their separation;

Dr. Maycock begins this subject, in the third *section* of his first paper, in the following manner: "It is an established fact, that from the *contact* and *separation* of dissimilar and insulated *metals* there is such a change in the *electrical state* of each *metal*, that, after the *separation*, the one is found to be *positive*, the others *negative*, in relation to surrounding bodies": Journ. vol. XXIX, p. 25. Before I proceed in this quotation, I must state the question to be decided, in order to direct to it the experiments. Dr. Maycock supposes, that there is no *electrical effect* produced during the *contact* of the *metals*; that it takes place only at their *separation*: Whereas I shall demonstrate by a great number of experiments, that these *effects* exist only during the *contact*, and that it is owing to extraneous circumstances that any effect remains after their *separation*.

but it is caused by their contact.

Dr. Maycock's apparatus

I come now to the experiments by which Dr. Maycock thinks to establish his system. "To determine this point," he says, "in place of the small *plate* which usually remains on my *electrometer*, I adapted a *copper plate* about 5 inches in diameter. It is evident, that, when this apparatus is placed on a common table, the *copper plate* will be connected with the wire of the *gold leaves*, but will be in every other respect perfectly *insulated*; and, consequently, that, whenever a state different from that of the surrounding bodies is produced in the *copper plate*, it will be indicated by the *divergence* of the *gold leaves*.

and experiments with it.

"The apparatus above described being so circumstanced, "that

"that the *tinfoil* of the *electrometer* was connected with the "Earth, while the *copper plate*, the wire, and the *gold leaves*, "were insulated, I brought, by means of an insulating "handle, a *zinc plate* also of 5 inches in diameter, into *contact* "with the *copper plate* on the electrometer; there was no "visible *divergence* in the *gold leaves*. On *separating* the "*metals*, the *gold leaves* immediately *diverged*. On again "bringing them into *contact*, if the charge of the *zinc plate* "had not been removed, the *leaves* returned to their natural "position. On again *separating* the *plates*, the *divergence* "took place as before....If the charge of the *zinc plate* "had been removed after the *separation*, the second contact "did not reduce the *gold leaves* to their natural state, but "left a slight *divergence* in them; and when the plates "were again *separated*, they diverged in a greater degree "than after the preceding *separation*.—Not, however, be-"yond certain limits, which apparently varied according to "the state of the *atmosphere* as to *moisture*." This cause of anomaly is possible, but very probably some other extraneous cause interfered in Dr. Maycock's operations; else *one single* contact and separation of his plates could not have produced a sensible *divergence* of the gold leaves. This is an interesting object, which I am going to explain.

Haüy's experiments the first.

Mr. Haüy, the celebrated mathematician and experimental philosopher, is the first who has proved, that, in the contact of *zinc* and *silver* (or copper), the former became *positive* and the latter *negative*: but from the account I have had of his experiments, it required about 10 *repetitions* of the operation, applied to a condenser, to make it sensible to the *gold leaves*.

These verified by the author.

It is probable that Dr. Maycock has not had the opportunity of being acquainted with these first and original experiments, else he would have found that they contradicted the results of his own: but, sir, he might have seen in pages 261 and 262 of my paper in your Journal for August 1810, that I have repeated them with insulated plates of *zinc* and *silver* 4 inches in diameter, and verified both parts of their result. My first view was to ascertain the effect of the contact of the *two metals* on their respective *electrical states*; an effect which appeared contradictory to my ob-

 servation

servation on the *galvanic pile*, related in p. 132 of your Journal for June 1810; for the extremity of this pile actually terminated by *silver* produced the *positive* divergence in the *gold leaves connected with it*; and the other, actually terminated by *zinc*, made them diverge as *negative*; which was the reverse of what Mr. Haüy had found in his experiments. I had discovered the cause of my illusion, as explained in the same paper; but this being an important point in *galvanism*, and wishing to ascertain it for my own conviction (not in view of Dr. Maycock's experiments, since I did not know them), I repeated the original experiments with the *plates* above mentioned.

Repeated contacts of the plates necessary to produce any sensible effect.

These experiments showed me first the certainty of the fundamental point, that, in their *contact*, the *zinc* plate became *positive*, and the *silver* plate *negative*. But the most important part of this experiment for my present purpose relates to the *small* effect, which *one single* operation produces on the *gold leaves*. Mr. Haüy having used *plates* as large as those with which Dr. Maycock has made his experiments, ten *repetitions* of the operation on his *condenser* were sufficient to produce a sensible *divergence* of the *gold leaves*; whereas my *plates* being only 4 inches in diameter, it required twenty *repetitions* of the alternate contacts with my *condenser*, to produce a sensible divergence of very narrow and long *gold leaves*. But besides, the experiments which follow that in my paper prove also, that a certain *number* of those *contacts* produce the same *electrical effect*, as the same *number* of *groupes* of the *two metals* remaining in *contact* with each other; to which object I shall return hereafter.

Dr. Maycock influenced by a different opinion.

The opinion, however, that *one single* operation with the two plates was sufficient to produce a sensible *electrical effect* seems to have influenced Dr. Maycock's *galvanic* system; for, in continuation of the above quoted part of his theory, he thus continues, p. 26: "The experiments, to which I "have just alluded, appear to be perfectly sufficient to "point out the fallacy of the explanation, which is very "generally received, of the *excitement* of the *galvanic pile*; "the whole of which rests on the assumption, that *dissimilar* "*metals*, while *in contact*, are in different *electrical states*, the

"the one being relatively *positive*, the other *negative*; which "has been shown to be perfectly *untenable*."

Though Dr. Maycock mentions here the *galvanic pile* invented by Volta, he does not appear to have used it in his *galvanic* experiments, for he never speaks but of the apparatus of *troughs*: but if he had seen my papers in your Journal, he would have found in them the same effects produced by the *galvanic pile*, in which the *two metals* remain in *contact* during its action. I shall therefore repeat here the most essential parts of these experiments, comparing them with his system; a comparison which, thus applied, will probably fix his attention.

Mr. De Luc's arrangement.

A plate in your Journal for June 1810 shows the construction of the *galvanic pile*, which I used in these experiments, and the paper explains my motives for that construction. My *pile* was composed of two columns, in which *ternal groups*, consisting of *zinc* and *silver* plates and *wet cloth*, were contained; but the order of the succession of the *metals* was inverse in the two columns; and thus, by means of a brass slip placed at the bottom, they were united in one *pile*, the *extremities* of which were both at the top of the apparatus, each connected with a *gold leaf* electrometer; which arrangement gave the facility of bringing the *chemical* experiments, in the *glass tubes with water*, more in sight.

His analysis of the effects of the pile.

I made the analysis of the effects of this *pile* by three different separations or *dissections* of its *ternal groups*, always composed of the *two metals* and a piece of *wet cloth*. These *separations* were produced by three small upright brass wires, forming the feet of brass *tripods*. In the first *dissection* the *ternal groups* separated by these *tripods* were the *two metals* and the *wet cloth* between them; and the effects of this *dissection* of the *pile* being the same as when it was not divided, I shall relate them first.

Experiments.

These experiments begin at p. 121 of my paper in your Journal. I first *wetted* the *cloth* with pure *water*: the *pile* produced the *divergence* of the gold leaves, and the *gasses* appeared in the *water* of the glass tubes; but not the *shock*; a very remarkable *galvanic* phenomenon, which Dr. Maycock has not considered, though it leads to the real cause of the *chemical* effects of the *pile*, namely, that a *liquid* must

separate the groups of two *metals*, in order to produce a *corrosion* on their surface. But the *corrosion* operated by pure *water* not having the effect of producing the *shock*, I undertook the series of experiments beginning by the 15th in the same paper, by which I found, that for the *shock*, the *corrosion* of the *metals* in the pile was to be produced by an *acid*. These are essential *galvanic* phenomena, which if Dr. Maycock had undertaken to explain, he would have found the deficiency of his system.

Corrosion by an acid requisite to the shock.

Separation of the electrical and chemical effects.

The second *division* of the *pile* led me to discover, that the cause of its *electrical excitement* is entirely different from that of the production of *chemical* effects. In this *dissection*, the groups of the *two metals* in *contact* were separated from each other, on one side by the *wet cloth*, and on the other by the *tripods*. In this construction, the *electrical excitement* was transmitted from group to group; for the *electrical* motions of the electroscopes at its extremities were very strong, and the *gold leaves* fell entirely when the extremities were connected together by the glass tubes with *water*; but no *chemical* effect was produced in that *water*, because the *corrosion* was not produced on the surface of both the *metals*, which is the essential cause of those effects, as it occasions a modification in the *electric fluid* itself. This is a most essential circumstance in *galvanism*; and as it is thus proved, and will be farther ascertained in the sequel, it must not be forgotten in forming theories on the effect either of the *pile*, or of the apparatus of *troughs*.

The corrosion modifies the electric fluid.

The electrical effects produced while the metals are in contact.

I return now to the question, whether the *electrical* effects produced by the association of *two* proper *metals* takes place during their *contact*, or only at their *separation*. If Dr. Maycock had read my paper in your Journal, No. 119, for August 1810, he would have seen, that the *two metals* remaining in *contact* in my *pile* produced both *electrical* and *chemical* effects; and that the *intensity* of these effects was proportional to the *number* of the groups thus connected. But he doubts whether the *course* of the *electric fluid* can be determined in the *pile* and its *circuit*; and he does not even think it necessary to inquire into the cause of the *electrical* effects, for he says in p. 14 and 15 of his paper in No. 131: "In the theory of Dr. Franklin, an *electric fluid* is supposed "to

Dr. M. supposes the seat of

"to be accumulated in the *glass*, and dissipated in the *seal-* "*ing-wax*. Admitting the existence of an *electric fluid*, it "would seem to follow, that if it be accumulated in the *glass*, "it must be dissipated in the *sealing-wax*: but as far as my "knowledge goes, it has never been determined, that it is "in the glass, and not in the *sealing-wax*, that the accumu- "lation takes place."

electricity not determined,

This opinion has led Dr. Maycock to suppose, that it cannot be determined whether a *fluid* does *enter* at a determined extremity of the *pile*, and *return* to the opposite extremity, when a *circuit* is established; but if he had seen my paper in your Journal for June 1810, he would have known, that I had made this point the object of a long series of experiments, demonstrating that, when the extremities of the *pile* are connected together by a *conducting* substance, the effects are produced by the circulation of the *electric fluid* entering the extremity, which, in the *pile* without a *circuit*, is *positive*, and returning to the extremity, which, in the same case, is *negative*.

and consequently its course in the pile not ascertained.

I come to another proposition of Dr. Maycock, p. 27 of the same Number of your Journal, relating to *galvanism*. "The *galvanic apparatus*," he says, "can only be *excited* "by a *decomposable fluid*, and this *fluid* is always *decomposed* "when the apparatus is *excited*." Sir H. Davy was at first of the same opinion; but I have demonstrated by various experiments in your Journal, that no *fluid* (or *liquid*) is necessary to produce that *excitation* with respect to the *electrical* phenomena of the pile; that the only condition of this effect, distinct from the *chemical* effects, is, that the groups of *two metals* in actual *contact* be separated by a *conducting* substance not *metallic*; and as it is a very essential point in *galvanism*, I shall briefly repeat its proofs.

Dr. M. supposes a decomposable fluid necessary to the excitement:

but the contrary has been shown.

This, first, is the cause why, in the second *dissection* of the pile above mentioned, where that condition only existed, the *electrical* phenomena continued; but not the *chemical* phenomena, which require, as I have proved, a *liquid* between the *metals*, in order to produce a *corrosion* on their surface. This is demonstrated by Exp. 19 in p. 135 of your Journal for June 1810. For, when before I had mounted a *pile* of 76 groups composed of *zinc* and *silver* plates 1·6 inch in diameter,

Experiment proving this.

in *contact* with each other, separated by pieces of *wet* cloth, that *pile*, beside the motions of the *gold leaves* at its extremities when free, produced *chemical* effects in the *water* of the *glass tubes* when forming the *circuit*; but, by substituting for the wet pieces of *cloth* pieces of the same *cloth*, new, and without *wetting* them, I had the same motions of the *gold leaves* at its extremities, only less, and not the smallest appearance of *chemical* effect in the *water* of the *glass tubes*.

Search for a conductor not metallic.

This first observation opened a new field to my view. Having conjectured, that *wool*, the material of the cloth I used, had in itself very little *conducting* faculty, and that it became a good *conductor* only by being *wet*, I undertook a long series of experiments for finding out what substance, not *metallic*, was the best *conductor*. I found in general, that *vegetable* substances were better *conductors* than *animal* ones; and among the former, plain writing *paper* having produced as much effect as any other, I fixed upon it, as being easily cut to the size of the metallic plates.

Vegetable best.

Experiment with this.

I then made the Exp. 20, which concludes the paper in your Journal. I mounted again the pile of 76 groups of *zinc* and *silver plates* in *contact*, separated only by pieces of *paper*. This *pile* produced as great *electrical* signs at its extremities, as that with the *wet cloth*, but not the smallest *chemical* effect appeared in the *water* of the *tubes*, when they connected these extremities. Thus were demonstrated the conclusions, which I had deduced from the phenomena of the *pile* itself.

Steps to which this discovery led.

My paper in your Journal for August 1810 describes the steps, to which this first discovery led me; which progress, had Dr. Maycock known it, would undoubtedly have struck him, as bringing to view an absolutely new field in experimental philosophy, not only by ascertaining the distinct causes of *electrical* and *chemical* effects in the *pile* (as indicated by the preceding experiment); but by this important phenomenon, that the *motions* of the gold leaves are very different at different times, without any connection with the difference of either *heat* or *moisture*; which changes were to be attributed to changes in the *electrical* state of the *ambient air*, from the following facts, leading to *meteorology*.

We

We have a method of comparing the *electrical* state of the *stratum* of *air* near the *ground* with that of the *strata* higher up, in which we can elevate a *conductor*; the comparative point of which, or the standard of *positive* and *negative*, is the *electrical* state of the ground; whereas, we have no such point of comparison within the *stratum* near the *ground*, in which our experiments are made; as the latter influences too much the state of the air near it; however I have observed changes in the *electrical* state of this lower *stratum* in the following manner. Having employed columns of many hundred groups, the *gold leaves* not only *diverged* very strongly, but they struck the tinfoils, fell, as being discharged, then rose and struck again. Now, the *number* of these alternate motions, in a given time, differ so much in different days, that I have seen sometimes 60 strikings in a minute, while at other times there was not even one in the same interval. This, as I have said and constantly observed, not having any connection with the changes of either *heat* or *moisture*, depends very probably on changes in the *electrical* state of the *ambient air*.

Electricity of the air near the ground and higher up may be compared.

Changes in the lower stratum also discoverable.

But let us fix our attention on the motions of the *gold leaves* in the electrometer, in order to understand the influence of the state of the *air* on the apparatus. On this essential point, Sig. Volta has made a very important step in electricity, which has removed the difficulties, till then insurmountable, in Dr. Franklin's system of *positive* and *negative*, without reference to any known *standard*. But Sig. Volta has first proved, that the particles of *air* possess the electric fluid as well as other bodies; and that the *electrical* state of the air in the place of observation is the *standard* of *positive* and *negative* with respect to the *electrometer*. Therefore, the *motions* of *gold leaves* indicate only the actual *electrical* state of the *air* which environs the instrument. It is impossible however to follow, in its phenomena, all the effects of the *electric fluid*, without a determination of its *nature*, which Dr. Maycock considers as unnecessary. I have given that determination in the same paper above mentioned, beginning at p. 254; but the phenomena on which it is founded are so numerous, that even in that paper I could only give a short account of them, referring to my works

Standard of positive and negative electricity discovered by Volta.

works, *Ideés sur la Météorologie*, and *Traité élémentaire sur le Fluide électro-galvanique.* In these works, I have demonstrated, by a long series of experiments, what I shall now summarily state on this subject,

The electric fluid a compound.

The *electric fluid* is composed of many *ingredients*, which however are only manifested when it exhibits *sparks*, by darting from one *conductor* to another. At this instant three phenomena are observed, which the *electric fluid* does not produce when it only moves along conductors; they are *light*, heat, and a peculiar *smell*. These sudden phenomena must be produced by a *decomposition* of the *electric fluid*; and in following the other phenomena attending this *decomposition* I have shown, that, beside the three *ingredients* thus manifested, *light*, *fire*, and an *odorate substance*, there are other *ingredients* in the *electric fluid*; one of which, well determined, is a most *tenuous fluid*, which imparts its strong expansibility to the others, and is the cause of the phenomenon called *electric influences*; a most characteristic effect of the *electric fluid*, which I have followed by exact experiments, beginning at p. 267 of the same paper. The *fluid* thus manifested was, for the facility of expression, to have a name; and I have called it *vector*, as giving *motion* to the *unexpansive* substance, which constitutes the *density*. Now, in the course of these experiments I have demonstrated, that *electrical motions* are produced only by the substance constituting the *density*, without any participation of the *fluid* producing the *electric influences*. This is an indisputable proof, that the *electric fluid* is a *compound* substance.

Similar effects from electricity and the pile.

There was another point to be determined in galvanism, which Dr. Maycock not having considered, his system remains without any foundation. The same effects as from the *galvanic pile*, namely the *shock* and the *gasses* in *water*, are produced by the *electric machine* charging a *battery* of *coated jars*; but in this case they are produced by a very much *condensed* electric fluid; while, when the same *fluid* has pervaded the *galvanic pile*, it produces these effects with an incomparably smaller quantity. With regard to this subject, I have proved by many experiments, what is above stated, that the *chemical* effects produced by the *pile* proceed

proceed from the *electric fluid* pervading it during the *corrosion* of the *metals* effected by a *liquid*; an alteration by which this *fluid*, though with a very small *density*, is *decomposed* when passing from one *conductor* to another. This important point, both in *electricity* and *galvanism*, is proved by a series of experiments beginning at p. 243 of the same paper.

Having been led by these experiments to the above-mentioned apparatus, wherein the groups of the *two metals* were separated by *writing paper*, it came into my mind to try, whether there would be any advantage for increasing the *electrical* effect of this new kind of *pile*, to fix, by pasting, that *paper* on one of the *metals*. I made this experiment on *zinc*, and *copper* or *silver*, the former of which becomes *positive* and the two other *negative* while in *contact*, and on *pewter*, which in contact with *zinc* becomes *negative*, and *positive* with *silver* or *copper*. An additional proof, that there is neither *positive* nor *negative* state belonging to any kind of body. The general result of these experiments, detailed in p. 245 of my paper, was, that there is a sensible increase of effect by pasting the *paper* on that *metal*, which in contact with the other becomes *negative* by losing some of its *electric* fluid and yielding it to the other; such as *silver* and *copper* with *zinc*. Effect of pasting paper on one of the metals.

This opening the prospect of obtaining a spontaneous and permanent *electric machine*, the power of which might be increased almost without limit by increasing the number of groups, I was going to undertake it in some measure, by pasting *paper* over copper plates of the same size of my *zinc* plates; when luckily it occurred to my recollection, that there was *paper* on which *copper* was ready laid, called *Dutch-gilt paper*. The experiment 27, p. 246 of my above-mentioned paper, relates my first trial. I constructed one of these new *piles* consisting of seventy-six groups of the same *zinc-plates* of 1·6 inch diameter, separated by equal pieces of *Dutch-gilt paper*, all the *copper* sides of which were turned towards the same extremity of the column. Thus I had seventy-six groups of *zinc* and *copper* in mutual *contact*, separated by the *paper* on which the *copper* was laid. Now, these seventy-six groups produced greater *electrical* effects Construction to which this led.

at their extremities, than the same number of groups of *zinc* and *silver* separated by the *wet cloth*: however now not the smallest *chemical* effect was produced in the *water* of the *glass tubes* when connecting these extremities, though the *gold leaves* fell; a proof of the *circulation* of the *electric fluid*.

Volta's condenser.

In p. 250 of the same paper, at exp. 29, begin some trials concerning the comparative *electrical* effects of the *size* and *number* of the *groups*, on which I had already formed my judgment, by Sig. Volta having explained to me at Paris, in 1782, the cause of the effect of his admirable instrument then lately invented, the *condenser*, as explained in that paper; according to which I found, that, for a mere *divergence* of the *gold leaves*, the *number* only of the *groups* determined it: but that, when they produced some other effect; as for instance to strike the sides, and thus be reduced to the *electrical* state of the *ground*; with the same *number* of *groups*, the *size* of the *plates* had an influence, as they repaired sooner what the *gold leaves* had lost; which made them strike more frequently in the same time, in proportion to the *size* of the *plates*.

Different effects of size and number.

Number of pairs of metals produce the effect of the same number of contacts of one pair.

I come now to the general results of these experiments, the account of which begins at p. 262 of the same paper; which will show what I have above mentioned, with respect to Dr. Maycock's system, that a certain number of groups, *zinc* and *copper* being in mutual *contact*, and separated by *paper*, produce sensibly the same *electrical* effect, as the same *number* of *contacts* of one insulated *metal*, after having been applied to the other while communicating with the *ground*. I made these experiments with a particular *condenser*, shortly described in my paper, and with plates of the same diameter as those of my column. I have given the general results of these experiments in some *tables*, p. 265, in all of which, A represents the *zinc* side, and B the *copper* side. In these *tables*, designed to trace the motion of the *electric fluid* through the *pile* in different circumstances, I have supposed the *pile* of the new construction, (which I have called *electric column*) of whatever number of *groups*, to be divided into eleven equal parts. I have used arbitrary *numbers* to express the *progress* of *positive* and *negative*, but they are proportional to the whole: I have made these *num-*

Results tabulated.

bers

bers increase regularly, though regularity cannot be expected in such experiments: but these *numbers* do not differ essentially from the immediate results. Results tabulated.

The first two columns, in that p. 265, represent the simultaneous progresses of *positive* and of *negative* in the *insulated* pile; one expressing the progress of the *negative* from A to B, and the other, the progress of the *positive* from B to A. These are as the elements of the combination of effects shown in the three following *tables*, in three different situations of the *pile*.

Table I.	Table II.	Table III.
Insulated pile.	B in communication with the ground.	A in communication with the ground.
A	A	A
+ 10	+ 20	0
+ 8	+ 18	— 2
+ 6	+ 16	— 4
+ 4	+ 14	— 6
+ 2	+ 12	— 8
0	+ 10	— 10
— 2	+ 8	— 12
— 4	+ 6	— 14
— 6	+ 4	— 16
— 8	+ 2	— 18
— 10	0	— 20
B	B	B

I cannot think, that Dr. Maycock has seen these experiments; for had he doubted their results at first, he would have found them confirmed in my following paper, with such evidence, that he could not have avoided, either to disprove them, or to show that they were not against his system.

I shall not enter into an account of all the experiments contained in this paper, as it would be a repetition in the same Journal. I had only here in view Dr. Maycock's system, which, according to my judgment, involves in obscurity the whole field of electricity and galvanism; I was therefore to recall those only of these experiments which relate to this subject: but they are more, sir, to this purpose, in my

my paper in your following number for October 1810, which I shall also recall for the same motive in a following communication, and I hope that the whole together will induce Dr. Maycock to change his system.

I am, with great regard, sir,

Your most obedient servant,

Windsor, J. A. DE LUC.

July the 10*th*, 1812.

VIII.

Explanation of a hydrostatical Phenomenon observed by FRANKLIN: *by* ROBINET*.

Agitation of water underneath oil.

IF you put water and oil into a tumbler, suspend the tumbler by a string, and give it a gentle swing, you will perceive nothing particular at the surface of the oil; but the surface of the water beneath will appear agitated, and form considerable waves. Such was the phenomenon observed by Franklin, and with which he was puzzled no doubt merely because he had not time to examine it: for that great man had so acute an eye in observing nature, that he scarcely ever failed to seize those connexions of facts, that constitute properly what we call physical laws. His confession of his ignorance on this occasion merely proves, that his modesty knew how to avail itself of his being too much occupied to examine every thing.

The phenomenon capable of variation.

This phenomenon is capable of assuming very different forms, by varying the circumstances by which it is produced. I shall confine myself here to that observed by Franklin, because it is one of the most complicated.

The facts result from two known principles.

The facts that compose this phenomenon, some of which are known to all the world, are the result of two hydrostatical principles, well known separately, but not yet considered together; that by which liquids seek their level, and that with the discovery of which Archimedes was so delighted.

Tendency of a fluid to a level.

By the first, all the parts of a liquid equally heavy, and perfectly movable on each other, tend toward the centre of the

* Abridged from the Journ. de Phys, vol. LXV, p. 277.

the Earth with equal energy; and, approaching it as long as they find no sufficient obstacle in the adjacent columns, do not stop till they arrive at that state, which is called the level. And as this motion is an effect of gravity, it is accelerated, carries all the parts beyond the point of equilibrium, and causes them to vibrate several times round this point, producing undulations, a kind of oscillations with which every one is familiar.

Motion of a body in a fluid.

By the second principle, a body moving in a liquid, being obliged, in displacing it, to communicate to it continually a part of its motion, is incessantly losing force; so that, in obeying the laws of gravitation, it falls through the liquid only with the excess of its specific gravity over that of the liquid in which it moves.

Application of these principles.

It might be supposed at first view, that this second principle should have no influence over either of the two liquids, that exhibit the phenomenon in question, because neither of them is properly in the other. Both, however, are subjected to this law: the lowermost, because its surface cannot acquire any undulatory movement without displacing the upper; and the uppermost, because its surface cannot move without raising the air, which then presses on it at every point. But, as we are accustomed to see the effects of this position with regard to the air, we do not think of referring them to their cause.

With respect to the inferior liquid, its situation and relation to the superior render a phenomenon very remarkable, which at the same time is essentially the same with what we see without attention at the surface of the superior.

Change of level in a fluid beneath another not much lighter.

To understand the reason of this singularity, let us suppose some cause to have disturbed the surface of the inferior liquid, so that it is no longer level, but a given column is a certain degree higher than another. This column does not exceed the shorter in weight by the whole quantity it is higher, for it makes only a part of the column that exists in the vessel at that point, consisting of the heavier liquid at its lower part, and the lighter at its upper. The shorter column of the inferior liquid in like manner is only the lower part of a column, the upper part of which is formed of the lighter liquid. The difference between these two columns is, the

the first has more of the heavier liquid and less of the lighter, the second more of the lighter and less of the heavier.

Conditions necessary to its restoration.

For these two columns to acquire their level it is necessary, that the first should lose a portion of the heavier liquid and acquire a portion of the lighter, and the second the contrary. And as there is no cause to produce this effect but the portion of the heavier liquid that one column has more than the other, the reduction of the inferior liquid to a level cannot be effected by the absolute gravity of the liquid as happens when it is alone in a vessel, but must be caused only by the excess of the weight of the inferior liquid over that of the superior.

The equilibrium more slowly restored,

Hence it follows in the first place, that, as the restoration of the level of the inferior liquid is the effect of a very small part of its gravity only, it must be extremely slow, and in consequence capable of being observed more easily, than when this liquid is alone in the vessel. It may not be amiss to observe however, that this cause, however small it may be, being a gravitating action, must retain its nature of an accelerating force, and thus produce an undulatory motion as in ordinary circumstances.

and its disturbance more conspicuous.

If now we attend to the interruption of equilibrium, or of the level, between the several columns of the inferior liquid, we shall find, that the same cause, which renders the restoration of the equilibrium slower and more obvious, renders its interruption likewise more considerable.

This scarcely observed with a single liquid.

Gravitation, as it exists before our eyes, imparts to ordinary bodies, in the shortest space of time we can estimate, a velocity very similar to those which we ourselves very commonly produce: so that when we do any thing to disturb the level of a liquid surface it is restored almost immediately. If we give a moderate inclination to a vessel filled with a liquid, the level is restored in proportion as we endeavour to destroy it; so that it requires some little knowledge of natural physiology to be aware, that it has been disturbed and restored.

Case of two liquids

But in the circumstances in which the inferior liquor is placed; as but a small portion of its gravity remains to reduce it to a level, it is evident, that it cannot effect this with the same promptitude; and that, if the same motion be employed

ployed to disturb its equilibrium, this will take place to a considerable degree, in a time when it would have been scarcely perceptible had there been but one liquid in the vessel. Thus, in the instance stated by Franklin, the swinging of the glass produces scarcely any agitation at the surface of the oil; because, though the glass inclines alternately to each side, as the motion is moderate, the surface of the oil returns to an equilibrium as fast as it is diverted from it. But the surface of the water, having to restore its equilibrium only the excess of its weight over that of the oil, which is very trifling, as we may estimate it at about 0·006 of the weight of the water, allows the small deviations from the level time to accumulate; so that this surface is no longer level when the swinging ceases, and is obliged to return to it by very slow and considerable undulations, that continue a long time

as observed by Franklin.

Of all the modes, in which this phenomenon may be varied, I shall mention but one, where the cause I have mentioned is too obvious to be mistaken.

The experiment varied.

Take a glass globe, mounted so as to be capable of being turned on its axis; put into it water alone, so as to fill it to a quarter of its diameter; and turn it gently: the water will continue apparently to occupy the lower part of the globe. Fill it then to three fourths, and the appearance will be the same, if you turn it in the same manner. So it will if you put oil alone, instead of water. Lastly, pour in water to one fourth of its height, and upon this oil to three fourths: then, if you turn the globe, there will be no change at the surface of the oil, and the whole body of liquid will appear to occupy the lower part of the globe. But with the water it will be different. When you have turned the globe a quarter round, you will perceive it nearly at the extremity of the horizontal diameter, instead of being in the lower part of the globe: and, if you then stop the rotary motion, the water will descend slowly down the side of the globe to the lower part, will ascend on the other side nearly to the same height, and thus oscillate a long time, till it settles at its lower part.

As I have said above we here see clearly, that the particular motion of the water under the oil has the particular character of that of solid bodies in fluids; and as it is the phe-

nomenon

nomenon of Franklin divested of all accessory complication; there can remain no doubt of its true cause.

This theory practically applied in common life.

They who frequently carry liquids in open vessels had at least an obscure perception of this theory. They know by experience, that the liquid is much less liable to be spilt by sudden movements, if a light body float on their surface. For this reason water-carriers put a wooden trencher into each of their buckets; and in vineyards a broom is put on the wine, when it is carried from the press to the cellar in an open wooden vessel. Any motion begun or terminated too suddenly would produce a considerable change of level in these liquids, a wave that would cause them to overflow. This wave is nearly prevented by the existence of the light body, that swims on the liquid; because all the columns, that terminate in this body, find in it an obstacle to their undulatory motion, as they can rise or fall only with this body itself: and as it corresponds to a great number of columns at the same time, and is urged in opposite directions by different columns, it is a considerable obstacle to them all; and thus it influences those columns it does not cover, since these cannot undulate separately from the others.

IX.

On the Nature of Sheep's Dung, and its use in dyeing Cotton the Red that is called India or Adrianople: *by* J. B. Vitalis, *Professor of Chemistry at Rouen**.

Process for dyeing cotton red.

THE process followed at present in our manufactories for dyeing cotton red, and which was first brought from the Levant, is composed of a series of operations, each of which requires to be elucidated by chemistry, if we would be certain of obtaining uniform success in this sort of dyeing.

Chemical examination of it.

Employed by government to teach the principles of chemistry in all its connexions with the useful arts, I thought it my duty to pay particular attention to that branch of industry

* Abridged from the Journ. de Phys. vol. LXVI, p. 153.

industry, which constitutes the base of the employment and trade of the first manufacturing city of the French empire.

Dyers of Rouen.

The manufacturers of Rouen employ both fast and false colours. I have already imparted to the latter, by the help of certain mordants, a degree of richness, lustre, and even permanence, before unknown; which no doubt procured the specimens I sent the honour of being admitted to the exhibition of 1806.

Articles of inferior price.

The class of citizens who are not wealthy, and they are the most numerous, require clothes of a price proportionate to their circumstances. Besides, the dyeing of inferior colours employs a number of workmen, and yields a profit, that would soon be seized by other towns, if it were despised here.

The best dyes most important,

and carried to great perfection.

But the reputation and wealth of our manufactories are derived chiefly from the colours called fast, that is to say those that are produced by the process for Adrianople red. These colours have opened a vast field of inexhaustible fertility to the manufacturer. He can now employ in his designs that variety, that happy mixture, that elegant association, that harmony of colours, which are so pleasing to the eye, and so gratifying to the taste of the most fastidious. Instead of those perishable colours, that delighted for a moment, the Indian red, and the extensive series of colours derived from it, as the cherry, rose, violet, lilac, julyflower, amaranth, &c., in all their various tints, have little to fear from the most destructive agents, and scarcely yield to the long continued action of air, light, and soap.

Some manufacturers fail in them.

This process therefore is of the highest importance to us; but, though it is practised with the greatest success by some manufacturers, others meet with obstacles, that occasion failures, which it would be highly useful to be able to prevent. I have endeavoured as far as possible to remove these, and to dissipate the uncertainty attending the operations performed on the cotton intended to receive this colour, by a chemical investigation of them.

Use of sheep's dung in dyeing red.

I have the honour now to present to the Institute the result of my examination into the nature and use of sheep's dung in dyeing Adrianople red, my object being to impart

 solid

solid notions of the mode of action and influence of the sheep's dung bath, the first applied to the cotton.

Mistaken opinion of it.

Various opinions have been broached on this subject; but the experiment, of which I am about to give an account, will at least dissipate every idea of its containing a large quantity of volatile alkali, to which La Pileur d'Apligny ascribes its property of *rosing* reds.

Sheep's dung distilled.

In May 1806 I distilled 61·19 gr. [945 grs] of fresh sheep's dung in a coated glass retort, to which I fitted a receiver furnished with a tube of safety, and a tube for collecting the gaseous products. The retort was placed in a reverberatory furnace, and gradually heated till the bottom was red.

Results.

On receiving the first impression of the fire, a very clear liquid passed over. On raising the heat, white vapours were evolved, oily, not very copious; and soon succeeded by drops of a very fluid oil, the colour of which was a very fine orange yellow. To this oil succeeded a second, thick, almost concrete, of a blackish brown, and smelling strongly empyreumatic. During the distillation about 50 cubic inches of elastic fluids passed over, which were found to be a mixture of carburetted hidrogen and carbonic acid.

Residuum.

Having broken the retort, I observed, that it was lined interiorly with a slight coating of coal, exhibiting the metallic lustre, and assuming on exposure to the air, though only in some places, the blue colour of prussiate of iron. At the bottom I found a dull black coal, tolerably dense, retaining the shape of the matter subjected to analysis, without any sensible taste, and exhaling a smell precisely like that of tobacco smoke.

This coal weighed 7·8 gram. Heated in a porcelain crucible it readily took fire, and before the vessel was redhot. I observed, that it emitted oily and empyreumatic vapours, owing no doubt to a small quantity of oil, with which it was still impregnated; and that it burned with a small white flame. After burning six hours with a fire well kept up, it left 3·68 gr. of a gray substance, which was found to be phosphate of lime.

Oils.

Of the two oils mentioned above I collected 3·91 gr.

Phlegm.

The coloured liquor in the receiver, contaminated with a few

few drops of fluid oil, weighed 48·8 gr. It turned sirup of violets green; at the same that it reddened infusion of litmus, though it is true but faintly.

The last-mentioned property was owing to a small portion of acetic acid, which was formed in the course of the distillation: I think its changing sirup of violets green may be ascribed to the presence of a small portion of gelatinous matter, that had passed over with the aqueous vapour, by which it was held in solution.

No ammonia.

For the rest, on assaying the liquor by every known method, no test discovered in it the least trace of ammonia.

From this experiment it appears, that 61·19 gr. of fresh sheep's dung yielded by distillation

Products.

An acid and alkaline liquor	48·80
Gaseous fluids	0·58
Concrete and fluid oil	3.91
Charcoal and phosphate of lime....	7·80
	61·09
Loss	0·1
	61·19

The dung contains more hidrogen than nitrogen.

From these results I think I may conclude, that sheep's dung contains much more hidrogen than nitrogen, which appears to me demonstrated, 1st, by the great quantity of water furnished by the matter analysed, and which certainly did not exist in it ready formed: 2dly, by the hidrogen gas collected under the jar: 3dly, by the oil obtained: and 4thly, by the absence of ammonia during the whole of the process.

It appears to me therefore proved, not only that ammonia does not exist in sheep's dung, but that it cannot be formed in it in large quantity.

Ammonia could not have the effect ascribed to it.

But let us go farther, and suppose for a moment, that sheep's dung contains a certain quantity of ammonia; is it not evident to all, who are acquainted with the process for Adrianople red, that this alkali, so volatile in its nature,

could not undergo the numerous manipulations and repeated dryings, either in the open air or by the heat of a stove, to which the cotton is subjected, without being entirely disengaged? Were it to be urged, that the alkali is rendered fixed by combining with the cotton; I should require proof of this, the contrary of which is shown by experiment.

Composition for brightening red.

But the property thus ascribed to ammonia of *rosing* cotton, that is of brightening the tint of madder red, and imparting to it warmth, lustre, and liveliness, is equally unfounded; for these effects can be produced only by forming with white marseilles soap and muriate of tin a metallic soap, in which the oxide of tin is held in solution by soda.

Sheep's dung useful by its albumen and gelatine.

Thus, since neither does ammonia possess the properties ascribed to it, nor is it contained in sheep's dung, we must look for the cause of its effects in some other principle. Now this can be nothing but the albumino-gelatinous matter so abundantly contained in sheep's dung: to convince ourselves of which, we have only to attend to the manner, in which it is used.

Mode of employing it.

In the first place the dung is macerated in a solution of soda, of the strength of about 4° [sp. gr. 1·027], for some time. The effect of this maceration is evidently the solution of the albumen and gelatine by means of the alkali. A certain quantity of this solution, passed through a sieve and diluted with a solution of soda at 2° [sp. gr. 1·013], is mixed with thick or mucilaginous olive oil; and thus a kind of liquid animal soap is formed, with which the cotton is carefully impregnated.

This impregnates the cotton with animal matter.

In this process the cotton, by combining with the albumen and gelatine, approximates to the nature of animal substances; which, as is well known, have a stronger attraction than vegetable substances for colouring matter. The combination appears to be farther promoted by the oily principle, that combines with the cotton at the same time.

Intestinal fluid of the sheep equally useful.

We now see why authors, who have written on India red, recommend the use not only of the dung, but also of the intestinal liquor of the sheep; which it would be much more advantageous to employ, were it possible to procure it in sufficient quantity for the demand.

The

The theory just laid down is supported by experiment. Having macerated fresh sheep's dung for four or five days in a lixivium of soda at 4°, I filtered, and obtained a reddish brown liquid. On separating the alkali by very dilute sulphuric acid, a copious, light precipitate was formed, which subsided to the bottom of the vessel, after having for some time occupied its whole capacity.

Sheep's dung macerated with alkali yielded a precipitate,

To remove all doubt respecting the nature of this precipitate, I collected it on a filter, washed it well with cold water, and then boiled it in a phial of pure water for near an hour. I then decanted off the liquid, which was of a reddish yellow, and poured into it a solution of tannin. This formed a precipitate, announcing sufficiently the presence of gelatine.

consisting of gelatine

The albumen, coagulated by the action of the heat, remained at the bottom of the phial in the form of little soft and spongy grumes: and to judge by the quantity of matter insoluble in water, though it was renewed three or four times, albumen abounds much more than gelatine in sheep's dung. I do not think it would be far from the truth to say, that the albumen is to the gelatine at least as three to one. Particular circumstances prevented my carrying the investigation to such a degree of accuracy, as I could have wished for my own satisfaction.

and albumen. The latter most abundant.

To establish a complete conviction on this subject, I shall add, that I tried an alkaline solution of whites of eggs, or albumen, instead of the sheep's dung bath; and that it succeeded completely in the preparation for both kinds of dyeing: all the colours were rendered much more permanent, than where natural or artificial sheep's dung baths were omitted.

An alkaline solution of albumen answered equally well.

This observation, founded on theory and experience, completely refutes the assertion of Le Pileur d'Apligny, that the dung and intestinal liquor of the sheep are of no use in fixing colours.

X.

X.

METEOROLOGICAL JOURNAL.

1812.	Wind	Pressure. Max.	Pressure. Min.	Pressure. Med.	Temperature. Max	Temperature. Min.	Temperature. Med.	Evap.	Rain	
6th Mo.										
June 2		29·98	29·95	29·965	68	53	60·5			☽
3	N E	30·02	29·98	30·000	65	46	55·5			
4		30·04	30·00	30·020	72	50	61·0	·38		
5	E	30·08	30·04	30·060	70	49	59·5			
6	N E	30·18	30·08	30·130	66	45	55·5			
7	N E	30·35	30·12	30·235	70	44	57·0	·52		
8	N E	30·40	30·35	30·375	62	46	54·0			
9	N	30·40	30·15	30·275	66	51	58·5			●
10	Var.	30·27	30·17	30·220	65	43	54·0	·36		
11	N W	30·07	30·03	30·050	75	53	64·0			
12	N W	30·03	29·93	29·980	74	48	61·0	·33		
13	S W	29·93	29·88	29·905	68	50	59·0			
14	S W	29·88	29·81	29·845	69	49	59·0			
15	S W	29·82	29·79	29·805	68	49	58·5		·07	
16	S W	29·79	29·58	29·685	65	48	56·5		·23	☾
17	S W	29·78	29·58	29·680	52	46	49·0		·39	
18	S W	29·58	29·49	29·535	59	53	56·0		·40	
19	S W	29·49	29·34	29·415	63	49	56·0	1·15	·09	
20	S W	29·53	29·33	29·430	60	46	53·0		·21	
21	S W	29·66	29·32	29·490	60	46	53·0		·16	
22	S W	29·83	29·66	29·745	60	43	51·5	·37	·01	
23	W	29·94	29·81	29·875	62	46	54·0		·05	
24	S W	29·94	29·91	29·925	59	45	52·0		·06	○
25	Var	29·91	29·60	29·755	63	50	56·5		·25	
26	Var.	29·86	29·45	29·655	58	42	50·0	·38	·61	
27	Var.	29·86	29·78	29·820	63	46	54·5		·22	
28	N	30·10	29·78	29·900	58	39	48·5		·01	
29	S W	30·03	29·96	29·995	64	48	56·0			
30	S W	29·86	29·70	29·780	62	52	57·0	·60	·05	
		30·40	29·32	29·881	75	39	55·87	4·09	2·81	

The observations in each line of the Table apply to a period of twenty-four hours beginning at 9 A.M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

NOTES.

NOTES.

Sixth Month. 3. A little rain at intervals. 4. A few large drops: cumulo stratus p.m. A shower to the S.W. Wind E. 6. Much dew: clear with cirrus. 6. Overcast, windy: then very fine with red cirri at sunset. 7. Cloudy morning: clear day afterward: brilliant orange twilight. 8. Cloudy: brisk wind. 9. Fair, with cumulus, and cirrus above: at sunset the wind rose, with some appearance of nimbus. 10. Cumulo-stratus, with a cold breeze all day. 11. A. m. wind fresh at W.: the maximum of temperature occured at nine: the barometer fluctuating. Cumulus clouds, with very large plumose cirri above, which showed red at sunset. The new moon appeared (in a white crescent, becoming afterward of a gold colour) in the midst of a pretty luminous twilight. 12. A.m. cloudy: barometer still unsettled: evening twilight luminous and orange coloured: a *stratus* began to appear at nine p. m. 13. A. m. misty: much dew. 15. Cool day: rather windy. 16. Rain last night: fair and cool. 17. Heavy short showers. 18. Fair, cloudy: rain by night. 19. The rainbow *twice* this morning. 21. Several hours' rain a. m. Barometer fluctuating. 22. Nimbi a. m. fair p.m. 23. Nimbi through the day: thunder twice to the S.W.: the wind veered as far as to N. W. but settled W. 24. A. m. much cloud: calm air: showers. 25. Cumulus, with very elevated cirrus in parallel bands E. and W. A *solar halo* for above two hours soon after noon, the higher atmosphere filled with cloud: at sunset the wind, which had been S. E. and S. W., came to N. W. 26. Cold stormy morning, wind N. Thunder twice about two p. m.: rain almost from sunrise to sunset. 27. A. m. sunshine: wind N. W.: a *solar halo* p. m. wind S. W.: evening wet and stormy. 28. A. m. wind N. a faint blush on the evening twilight. 30. Windy evening: rain at intervals.

RESULTS.

Winds variable, the South-west most continuous.

Barometer: highest observation 30·40 inches; lowest 29·32 inches; Mean of the period 29·881 inches.

Thermometer: highest observation 75°; lowest 39°; Mean of the period 55·37°.

Evaporation 4·09 inches. Rain 2·81 inches.

This period is remarkable for being pretty equally divided into a dry and a wet moiety: the former commencing with the first quarter, the latter the day before the last quarter of the moon. The return of the first quarter appears (by subsequent observations) to have again nearly coincided with that of dry weather.

PLAISTOW. L. HOWARD.

Seventh Month, 15, 1812.

XI.

XI.

An Account of "The Sulphur," or "Souffrière" of the Island of Montserrat: by NICHOLAS NUGENT, M. D. *Hon. Member of the Geological Society**.

Occasion of the visit.

ON my voyage last year (October 1810) from Antigua to England the packet touched at Montserrat, and my curiosity having been excited by the accounts I received of a place in the island called "The Sulphur," and which, from the descriptions of several persons, I conceived might be the crater of an inconsiderable volcano, I determined to avail myself of the stay of the packet to visit that place.

Face of the island.

The island of Montserrat, so called by the Spaniards from a fancied resemblance to the celebrated mountain of Catalonia, is every where extremely rugged and mountainous, and the only roads, except in one direction, are narrow bridle paths winding through the recesses of the mountains; there is hardly a possibility of using wheeled carriages, and the produce of the estates is brought to the place of shipment on the backs of mules. Accompanied by a friend, I accordingly set out on horseback from the town of Plymouth, which is situate at the foot of the mountains on the seashore. We proceeded by a circuitous and steep route about six miles, gradually ascending the mountain, which consisted entirely of a uniform porphyritic rock, broken every where into fragments and large blocks, and which in many places was so denuded of soil, as to render it a matter of astonishment how vegetation, and particularly that of the cane, should thrive so well. The far greater part of the whole island is made up of this porphyry, which by some systematics would be considered as referrible to the newest floëtztrap formation, and by others would be regarded only as a variety of lava. It is a compact and highly indurated argillaceous rock of a gray colour, replete with large and perfect crystals of white feldspar and black hornblende. Rocks of this description

Journey to the Sulphur.

* Trans. of the Geol. Soc. vol. I, p. 185.

generally

generally pass in the West Indies by the vague denomination of fire stone, from the useful property they possess of resisting the operation of intense heat. A considerable quantity of this stone is accordingly exported from Montserrat to the other islands which do not contain it, being essential in forming the masonry around the copper boilers in sugar works. We continued our ride a considerable distance beyond the estate called "*Galloway's*," (where we procured a guide) till we came to the side of a very deep ravine which extends in a winding direction the whole way from one of the higher mountains to the sea. A rugged horse-path was traced along the brink of the ravine, which we followed amidst the most beautiful and romantic scenery. At the head of this ravine is a small amphitheatre formed by lofty surrounding mountains, and here is situate what is termed "The Sulphur." Though the scene was extremely grand and well worthy of observation, yet I confess I could not help feeling a good deal disappointed, as there was nothing like a crater to be seen, or any thing else that could lead me to suppose the place had any connexion with a volcano. On the north, east, and west sides were lofty mountains wooded to the tops, composed apparently of the same kind of porphyry we had noticed all along the way. On the south, the same kind of rock of no great height, quite bare of vegetation, and in a very peculiar state of decomposition. And on the south-eastern side, our path and the outlet into the ravine. The whole area thus included might be three or four hundred yards in length, and half that distance in breadth. The surface of the ground, not occupied by the ravine, was broken and strewed with fragments and masses of the porphyritic rock, for the most part so exceedingly decomposed as to be friable and to crumble on the smallest pressure. For some time I thought that this substance, which is perfectly white and in some instance exhibits an arrangement like crystals, was a peculiar mineral; but afterward became convinced, that it was merely the porphyritic rock singularly altered, not by the action of the air or weather, but, as I conjecture, by a strong sulphureous or sulphuric acid vapour, which is generated here, and which is probably driven more against one side

West India firestone.

The Sulphur described.

No appearance of a volcano.

The rock decomposed by the sulphureous vapour.

by

by the eddy wind up the ravine, the breezes from any other quarter being shut out by the surrounding hills*.

This evolved from fissures

Admidst the loose stones and fragments of decomposed rock are many fissures and crevices, whence very strong sulphureous exhalations arise, and which are diffused to a considerable distance; these exhalations are so powerful as to impede respiration, and near any of the fissures are quite intolerable and suffocating. The buttons of my coat, and some silver and keys in my pockets were instantaneously discoloured. An intense degree of heat is at the same time evolved, which, added to the apprehension of the ground crumbling and giving way, renders it difficult and painful to walk near any of these fissures. The water of a rivulet, which flows down the sides of the mountain and passes over this place, is made to boil with violence, and becomes loaded with sulphureous impregnations. Other branches of the same rivulet, which do not pass immediately near these fissures, remain cool and limpid, and thus you may with one hand touch one rill which is at the boiling point, and with the other hand touch another rill which is of the usual temperature of water in that climate. The exhalations of sulphur do not at all times proceed fimthe same fissures, but new ones appear to be daily formed, others

with intense heat.

Boiling rivulet.

Fissures continually varying.

* This peculiar decomposition of the surrounding rock has been frequently observed in similar situations, and under analogous circumstances, and has I find been accounted for by other persons in the same way: thus Dolomieu says, "The white colour of the stones in the interior of all the burning craters is owing to a real alteration of the lava produced by acid sulphureous vapours, which penetrate them, and combine with the alumine that constitutes their base, thus forming the alum obtained from volcanic substances." Voy. aux Isles de Lipari. p. 18.

And he afterward adds, "The alteration of lavas by acid sulphureous vapours is a kind of analysis of volcanic substances made by Nature herself. There are lavas, on which the vapours have not yet had sufficient time to act, so as to change their nature entirely; and then we see them in different states of decomposition, which we know by the colour."

Alum is doubtless formed at this place, as well as elsewhere under similar circumstances: the potash necessary for the composition of this salt being, as well as the argil, derived from the surrounding rock. See Vauquelin's Memoire. Journal des Mines, vol. x, p. 441.

becoming

becoming, as it were, extinct. On the margins of these fissures, and indeed almost over the whole place, are to be seen most beautiful crystallizations of sulphur, in many spots quite as fine and perfect as those from Vesuvius, or indeed as any other specimens I have ever met with. The whole mass of decomposed rock in the vicinity is, in like manner, quite penetrated by sulphur. The specimens which I collected of the crystallized sulphur, as well as of the decomposed and undecomposed porphyry, were left inadvertently on board the packet at Falmouth, which prevents my having the pleasure of exhibiting them to the society. I did not perceive at this place any trace of pyrites, or any other metallic substance, except indeed two or three small fragments of clay iron stone at a little distance, but did not discover even this substance any where *in situ*. It is very probable that the bed of the glen or ravine might throw some light on the internal structure of the place, but it was too deep, and its banks infinitely too precipitous for me to venture down to it. I understood there was a similar exhalation and deposition of sulphur on the side of a mountain not more than a mile distant in a straight line; and a subterranean communication is supposed to exist between the two places.

Sulphur beautifully crystallized.

No trace of pyrites.

Another Sulphur a mile distant.

Almost every island in the western Archipelago, particularly those which have the highest land, has in like manner its "Sulphur" or, as the French better express it, its "*Souffrière*." This is particularly the case with Nevis, St. Kit's, Guadaloupe, Dominica, Martinico, St. Lucia, and St. Vincent's. Some islands have several such places, analogous I presume to this of Montserrat; but in others, as Guadaloupe, St. Lucia, and St. Vincent's, there are decided and well characterized volcanoes, which are occasionally active, and throw out ashes, scoriæ, and lava, with flame. The volcano of St. Vincent's is represented by Dr. Anderson, and others who have visited it, as extremely large and magnificent, and would bear a comparison with some of those of Europe. These circumstances appear to have been entirely overlooked by geologists in their speculations concerning the origin and formation of these islands. It has indeed occurred to most persons, on surveying

Most islands in the western Archipelago have one or more,

and some have volcanoes.

General remarks on the island.

surveying the regular chain of islands, extending from the southern Cape of Florida to the mouths of the Orinoco, as exhibited on the map, to conclude that it originally formed part of the American Continent, and that the encroachments of the sea have left only the higher parts of the land, as insular points, above its present level. But this hypothesis, however simple and apparently satisfactory in itself, will be found to accord very partially with the geological structure of the different islands. Many of them are made up entirely of vast accretions of marine organized substances; and others evidently owe their origin to a volcanic agency, which is either in some degree apparent at the present time, or else may be readily traced by vestiges comparatively recent. There is every reason to believe, however, that some of the islands are really of contemporaneous formation with the adjacent parts of the continent, from which they have been disjoined by the incursions of the sea, or by convulsions of nature, and it is probably in those islands which contain primitive rocks, that we are chiefly to look for a confirmation of this supposition.

XII.

Account of various specimens of Natural History brought from the Island of Java, Madura, Bali, &c.; by Mr. LESCHENAULT*.

The author sails for New Holland,

MR Leschenault was one of the scientific persons, who sailed with captain Baudin, to investigate the natural history of New Holland and the adjacent countries. It is well known, that most of those gentlemen died; but the zeal and talents of the survivors rendered this voyage one of the most interesting to science.

leaves the ship at Timor, and repairs to Java.

Mr. Leschenault, being obliged to quit the ship at Timor, in may, 1803, on account of sickness, went over to Java, and repaired to Samarang, the chief seat of the Dutch government, and less insalubrious than Batavia. Governor Engel-

* Journ. de Phys. vol. LXV, p. 406.

hard,

hard, a very well informed man, received him very courteously, and afforded him every assistance in his researches.

Tour in Java.

On the 24th of october Mr. L. set off from Samarang for Sourakorta, the residence of the emperor of Java, and about sixty miles south of the former place. On his journey he visited the mountains of Dounarang, Morbabou, Telomayo, and Marapi. The last has on its summit a volcano constantly emitting smoke.

Mountains. Volcano.

From Sourakorta he repaired to Djiokikorta, the residence of the sultan of Java. The sultan and the emperor are two independant princes. On this road, which is little more than forty miles, are some ruins of ancient temples, remarkable for their extent. Among these are a number of statues of lava, which seem to prove, that the people followed the religion of the bramins.

Ancient temples. Statues.

A severe illness obliged him to return to Samarang. When he recovered, he visited the other parts of the island. After sailing to Madura, he returned to Java, and visited mount Idienne; a volcano, in which he found a lake, the water of which was strongly impregnated with sulphuric acid. He afterward sailed to the island of Bali.

Volcano and sulphuric lake.

Having returned to Samarang, and packed up his collections, he repaired to Batavia in october 1806; sailed thence on the 27th of november on board an American vessel; arrived at Philadelphia in april 1807; sailed thence in june; and landed safe in France in july.

Return home.

The following is an abstract of the account of his collection given to the museum of Natural History by Messrs. Cuvier, Desfontaines, and Lamarck.

His collection.

We shall say nothing, observe these gentlemen, of the weapons, garments, and other articles used by the Indians, or of two very curious statues found in the ruins of a temple, as they do not pertain to natural history, and will find their place among the antiquities of the imperial library.

Articles not belonging to natural history.

But Mr. L. has brought some articles interesting to the history of man: as some fragments of undoubtedly human bones brought from a burying place, that appear to have undergone at least a commencement of calcareous infiltration; and a scull of a Chinese of Java, that will increase our collection of those of different nations.

Fossil human bones. Skull.

Among

New species of ape. Among the quadrumanous animals he has brought a black ape of a new species, with its young, and its skeleton; and the great slowpaced lemur [*le grand lori paresseux*], also with its skeleton.

Galeopithecus. You know how rare the flying macauco, or pretended lemur volans of Linneus, is in collections. Neither Buffon nor Linneus ever saw it. Mr. L. has brought four of different ages, and two skeletons. The red and the variegated of some recent naturalists are only differences of age.

Bats, viverræ, felis. He has five or six species of bats, two of which, at least, appear new to us; a new weasel; a new civet; and a new species of felis, in size approaching the lynx.

Skunk. His most curious quadruped in our opinion is a new skunk [mouffette], truly belonging to that genus, hitherto supposed peculiar to America, like it striped with white on a black ground, but distinguished from the other species by being without a tail. It is common in the island of Java, and emits when pursued the same stinking smell as other skunks.

Squirrels and ichneumon. He has also a new flying squirrel, a new ichneumon scarcely as big as a rat, and a new squirrel; beside many specimens of the Java squirrel, and of the taguan, or greatest flying squirrel.

Skeletons. To these he has added the skeleton of a porcupine of Java, and those of two musks, which were wanting to your anatomical collection.

Birds. Of birds Mr. Leschenault has brought over 130 species, which we have not been able to examine with sufficient minuteness to say how many are new.

Wild cocks. There are however two different species of wild cocks, with their hens: one was discovered by Sonnerat, the other appears to us new. And we noticed a new bird of Paradise, black, with a very shining throat, among four other species.

Bird of Paradise.

Snakes. Of reptiles Mr. L. has brought a superb skeleton of a serpent, more than 15 feet [16f. Eng.] long, worthy a place in the finest collection. A specimen scarcely inferior to it, at least in rarity, is a well preserved skin of the celebrated achrocordus, or warted snake of Java. With these are about 30 other species of snakes, and several lizards; among which are the gecko of Java, and the blue galeot with its spindle-shaped eggs.

and lizards.

The fishes, molluscæ, worms, and zoophytes are less numerous, and their collection less important in proportion. We have remarked however two new seapens, one of which is extremely curious, on account of its long and slender shape, the other on that of its large spines. Pennatulæ.

In the class of insects however he has been more successful, having at least 600 specimens of 200 different species, more than a third of which are new, and the rest are valuable, and fetch good prices. They are all in excellent preservation; and his buterflies in particular are very numerous, and admirably fresh in their colours. Insects.

He has also many shells, some of which are interesting. Shells.

His herbal is composed of 900 plants nearly, about a fourth of which are new. He has already drawn up descriptions of near 700, and has made drawings of near 100. They are a valuable acquisition to botany. Plants.

He has brought about 200 species of seeds, which will be divided between the garden of the empress at Malmaison, and that of the Museum. Thirty species too have been brought growing from North America, and are intended for the garden at Malmaison. Seeds.

Of mineralogy he collected in the island of Java some very fine specimens of fossil wood, changed to the siliceous state, without the annual rings having disappeared: a deep green jasper, of a very fine grain, useful to the lapidary: and a collection of lavas and specimens of sulphur from mount Idienne. Minerals.

This mountain, which is about 1100 toises [2344 yds] above the level of the sea, Mr. L. ascended with much danger and difficulty, accompanied by *commandant* Wikerman, to ascertain whether the sulphur produced by the volcano might not be turned to account; and particularly to investigate the cause, that affects the waters of the White river at certain seasons of the year, and render them noxious to men, animals, and even vegetation. Volcano of mount Idienne.

This cause did not escape him, and he found it dependant on a curious volcanic fact. On arriving near the summit of the crater, which appears to be changed at present into a *solfaterra*, he descended to the bottom of this cavity, which is about 400 feet [426 f. Eng.] deep, and 250 toises Lake on it occasionally spoils the water of the White river.

[533 yds] across the widest part of its bottom. Here he perceived four openings, or mouths, near the top of the cavity, continually emitting clouds of sulphurous acid vapour, which, being condensed by the action of the cold air, fell into a great lake at the bottom, which is contained in the crater of the ancient volcano.

The waters in this basin, thus continually impregnated with the vapour, become so acid, that they attack every thing they touch; altering all the adjacent lava, and forming sulphate of iron and of lime, which they hold in solution, as well as sulphate of alumine. Accordingly when the rainy season arrives, the lake swells, overflows, and contaminates the water of the White river.

This may be obviated.

The cause being thus known, it is easy to obviate the noxious mixture of this water, by turning aside that which descends from the lake at certain seasons; and opposing obstacles sufficient to prevent its reaching the White river, which would thus remain constantly wholesome. This is a service of no small importance to the colony.

Analysis of the water.

Mr. Vauquelin has analysed the acid water of this lake, and found in it sulphuric acid, sulphurous acid, muriatic acid, sulphur, sulphate of potash, alum, and sulphate of iron.

XIII.

Analyses of Minerals: *by* MARTIN HENRY KLAPROTH, *Ph. D. &c.*

(Continued from p. 161).

Meadow iron ore.

WIESENERZ (meadow iron ore).

Black oxide of iron	66
Oxide of manganese	1·5
Phosphoric acid	8
Water	23
	98·5

Granular iron ore.

Pisiform ironstone from Hogau.

Oxide of iron	53
Silex	23
Alumine	6·5
Oxide of manganese	1
Water	14·5
	98

Granular

Granular chromated iron from Styria. Chromate of iron.

Oxide of chrome	55·5
——— iron	33
Alumine	6
Silex	2
Loss in roasting	2
	98·5

Black manganese from Klapperud in Dalecarlia. Manganese.

Oxide of maganese	60
Silex	25
Water	13
	98

Cerite from Bastnaes in Sweden. Cerite.

Oxide of cerium	54·50
Silex	34·50
Oxide of iron	3·50
Lime	1·25
Water	5
	98·75

A fire-coloured opal, brought by Humboldt from Zimapan, in Peru. Fire-coloured opal from Peru.

Silex	92
Water	7·75
Oxide of iron	0·25
	100

Brazilian topaz. Brazilian topaz.

Silex	44·5
Alumine	47·5
Oxide of iron	0·5
Fluoric acid	7
	99·5

Saxon topaz. Saxon topaz.

Silex	35
Alumine	59
Fluoric acid	5
Oxide of iron *a trace*	1
	100

 Crystallized

Zoisite.

Crystallized zoisite.

Silex	45
Alumine	29
Lime	21
Oxide of iron	3
	98

Augite from Carniola,

Lamellar augite from Carniola.

Silex	52·50
Magnesia	12·50
Lime	9
Alumine	7·25
Oxide of iron	16·25
Potash	0·50
	98

and Sicily.

Scoriform augite from Sicily.

Silex	55
Alumine	16·50
Oxide of iron	13·75
Lime	10
Magnesia	1·75
Water	1·50
Manganese *a trace*	
	98·5

Apatite.

Conchoidal apatite, or spargelstein, from Zillerthal.

Lime	53·75
Phosphoric acid	46·25
	100

Columnar brownspar.

Stanglichen braunspath (columnar brownspar), brought by Humboldt from Valenciana of Guanaxuato, in Mexico.

Carbonated lime	51·5
——— magnesia	32
——— iron	7·5
——— manganese	2
Water	5
	98

Dolomite.

Dolomite from St. Gothard. Dolomite from St. Gothard,

Carbonated lime	52
—— magnesia	46·50
Oxided iron	0·50
—— manganese	0·25
Loss	0·75
	100

Dolomite of the Appennines:

A decomposed dolomite from Castelamare. from Castelamare, and

Carbonated lime	59
—— magnesia	40·5
Loss	0·5
	100

A dolomite in mass.

Carbonated lime	65
—— magnesia	35
	100

A dolomite from Carniola. from Carniola.

Carbonated lime	52
—— magnesia	48
Oxided iron	20·0*

Blue anhydrite, called muriacite, from Sulz on the Necker, spec. grav. 2·94. Anhydrous sulphate of lime from Sulz,

Lime	42
Sulphuric acid	57
Oxided iron	0·10
Silex	0·25
	99·35

Compact anhydrite, vulgarly tripestone, from Bochnia. Bochnia, and

Lime	42
Sulphuric acid	56·50
Muriate of Soda	0·25
	98·75

* Probably 0·20; although the other component parts, exclusive of this, amount to 100. C.

Hall in Tyrol.

Anhydrite from Hall, in Tyrol.

Lime	41·75
Sulphuric acid	55
Muriate of Soda	1
	97·75

agnesian spar.

Bitterspath (magnesian spar) from Hall in Tyrol.

Carbonated lime	68
——— magnesia	25·5
——— iron	1
Water	2
A mixture of clay	
	96·5

Terre verte from Verona,

Terre verte from Mount Baldo near Verona.

Silex	53
Oxided iron	28
Magnesia	2
Potash	10
Water	6
	99

Cyprus,

Terre verte from Cyprus.

Silex	51·5
Oxided iron	20·5
Magnesia	1·5
Potash	18
Water	8
	99·5

and West Prussia.

Terre verte from New West Prussia.

Silex	51
Alumine	12
Lime	3·5
Magnesia	2·5
Oxided iron	17
Soda with a suspicion of potash	4·5
Water	9
	99·5

Alumstone

Alumstone from la Tolfa.

Alumstone from la Tolfa,

Silex	59·5
Alumine	19
Sulphuric acid	16·5
Potash	4
Water	3
	102

Alumstone from Hungary.

Hungary,

Silex	62·25
Alumine	17·50
Sulphuric acid	12·50
Potash	1
Water	5
	98·25

Aluminous earthy schist from Freyenwalde.

and Freyenwalde.

Sulphur	28·5
Charcoal	196·5
Alumine	160
Silex	400
Black oxide of iron, with a trace of manganese	64
Sulphate of iron	18
Gypsum	15
Magnesia	2
Sulphate of potash	15
Muriate of potash	5
Water	107·5
	1011·5

Jade from Switzerland, hemanite of Delamétherie, saussurite of Saussure.

Jade.

Silex	49
Alumine	24
Lime	10·50
Magnesia	3·75
Oxide of iron	6·50
Soda	5·50
	99·25

Lazulite

Lazulite.

Lazulite from Krieglach in Styria.

Alumine	71
Silex	14
Magnesia	5
Lime	3
Oxide of iron	0·75
Potash	0·25
Water	5
	99

Moya.

Moya from Quito, brought over by Humboldt, 100 grs. yielded

	Cubic inches.	
Carbonic acid gas	2·25	= 1·06
Hidrogen gas	14·50	= 0·36
Water and ammonia with some empyreumatic oil		11
Charcoal		5·25
Silex		46·50
Alumine		11·50
Lime		6·50
Oxide of iron		6·21
Soda		2·50
		90·88

Guano.

Guano from the islands on the coast of Peru, brought over by Humboldt. This guano is supposed to be the remains of the excrements of the birds, with which those islands are covered.

Ammoniacal uric acid	16
Phosphated lime	10
Oxalated lime	12·75
Silex	4
Muriated soda	0·50
Mixed sand	28
Water with an animal residuum, and loss	26·75
	98

Klebschiefer,

Klebschiefer, or polishing slate, from Menilmontant. Polishing slate.

Silex	62·50
Lime	8
Oxide of iron	4
Charcoal	0·75
Alumine	0·50
Lime	0·25
Water and gasses evolved	22
	98

Olive-green garnets from Siberia. Olive green garnets.

Silex	44
Lime	33·5
Alumine	8·5
Oxide of iron	12
——— manganese, *a trace*	
Loss	2
	100

Green chalcedony from Olympus, near Prusa, in Asia minor. Green chalcedony.

Silex	96·75
Oxide of iron	0·50
Alumine	0·25
Water	2·50
	100

True Lemnian earth. Lemnian earth.

Silex	66
Alumine	14·50
Oxide of iron	6
Lime	0·25
Magnesia	0·25
Soda	3·50
Water	8·50
	99

Fuller's

Fuller's earth, English,

Fuller's earth from England.

Silex	53
Alumine	10
Oxide of iron	9·75
Magnesia	1·25
Lime	0·50
Sea salt	0·10
Water	24
Potash, *a trace*	
	98·6

and Siberian.

Red fuller's earth from Siberia.

Silex	48·5
Alumine	15·5
Magnesia	1·5
Oxide of iron	6·5
——— manganese	0·5
Water	25·5
Sea salt, *a trace*	
	98

Red earth of Sinope.

Earth of Sinope, in Pontus, brought from Natolia by Mr. Hawkins. This earth, according to Pliny, formed red paint.

Silex	32
Alumine	26·5
Oxide of iron	21
Sea salt	1·5
Water	17
	98

Tincal.

Tincal, crystallized borax.

Boracic acid	37
Soda	14·5
Water of crystallization	47
	98·5

Datholite

Datholite. Datholite.

Silex	36·5
Lime	35·5
Boracic acid	24
Water	4
Iron and manganese, *a trace*	
	100

Fluor. Fluor.

Lime	67·75
Fluoric acid	32·15
Oxide of iron, *a trace*	
	99·9

(To be continued.)

SCIENTIFIC NEWS.

Geological Society.

JUNE the 5th. An account of some new varieties of Alcyonia found in the Isle of Wight by Thomas Webster, Esq. Member of the Geological Society, was read. In viewing the rocks about Ventnor Cove, and in various parts of the undercliff, Mr. Webster remarked, in the sandstone stratum immediately under the chalk marl, a great number of small prominences, resembling in form the branches of trees. They were of various sizes, from half an inch to three or four inches in diameter; their substance was sandstone, of the same kind as the rock they were in; but the part resembling the bark was somewhat harder, which enabled it to endure longer than the rest of the stone, and thus project above its surface. Some of them were straight, others a little crooked, and in a few instances he observed them forked. He found fragments of these bodies in every part of the island, where the sandstone stratum can be seen, and particularly among the masses of rock lying under the cliffs of Western Lines. In this last place he found, that the stems above described had frequently heads or bulbous terminations attached to them, in form somewhat resembling a closed tulip; and in some of these he found distinct traces of organic

New varieties of alcyonia.

ganic structure; from which it appeared, that these heads consisted of a group of *tubuli*, now converted into, and enveloped with stony matter. Beside these extraordinary shapes, which projected in relief, Mr. W. observed a variety of very regular white figures, as if painted upon the rock, being even with the surface. They consisted of circles from two inches to half an inch in diameter, ellipses of various eccentricities, and parallel lines both straight and curved. By a careful examination Mr. W. found, that these white figures belonged to the other class of bodies already described; and that the cylinders were only the internal parts of the same body, the sections of which formed the white circular and elliptical figures. The vast masses of rock, which have fallen down, having separated from the cliff at the divisions between the beds, showed their upper and under surfaces covered with layers of these bodies heaped upon each other, and lying prostrate in every possible direction: and in the joints between the beds, where they were still not separated, they were distinctly seen. The green sandstone and the limestone he found to be the chief repositories of these bodies; in the ferruginous sand below the green sandstone he found none, and only a few fragments of cylinders in the blue marl on which the sandstone rests. He traced them upwards into the chert, but they there became rare, and they totally disappeared in the chalk marl. He found them however frequently in the fragments of flint lying on the shore. Mr. Webster having brought away an extensive series of specimens, which he has since deposited in the collection of the Society, submitted them to the examination of Mr. Parkinson, who is of opinion, that they belong to the genus *alcyonium*, but that they are of three or four different species, neither of which has been hitherto described. From the resemblance which these bodies bear to a closed tulip attached to its stalk, Mr. Webster suggests, that the name of *tulip alcyonium* may not be improperly applied.

Hippurites from Sicily. Some observations by James Parkinson, Esq. Mem. G. S. on the specimens of *Hippurites* from Sicily, presented to the Society by the Hon. Henry Grey Bennet, Mem. G. S. were read. These specimens Mr. P. considers to be such as demand particular attention, as they possess those characters,

racters, which will probably serve to correct some erroneous opinions respecting the nature and habits of the animals of which these shells were the dwellings. One of the specimens contains a nearly perfect shell, longitudinally divided so as to display the two ridges, with the numerous septa and chambers. From an examination of the specimens, and by comparing them with the observations he has before had an opportunity of making, Mr. Parkinson is of opinion, that the structure of the shell of the hippurites is such, as would enable the animal to raise itself to the surface of the water. This opinion is in opposition to that of Mr. Denys de Montfort, and most of the French oryctologists, who consider the hippurites as belonging to what they term *pelagian shells*, or such as constantly inhabit the bottom of the sea, never rising to the surface, or appearing on the shore; and therefore, that there is no reason to suppose them belonging to animals which are now extinct; but only, that their recent analogues have not yet been brought to view.

Island of Barbadoes.

June the 19th. A paper by Joseph Skey, M. D., entitled "Some remarks upon the Structure of Barbadoes as connected with specimens of its Rocks," communicated by Arthur Aikin, Esq., sec. was read; together with a note by Mr. Parkinson on some of the specimens presented by Dr. Skey. The island of Barbadoes is totally unlike those immediately near it, both in structure and in appearance, the land rises in a gentle swell from the coast towards the middle of the island, except in one small district: its highest hills do not exceed 800 or 900 feet, and their general direction is nearly N. W. and S. E. Upon the N. Eastern coast the shores are bolder than in the other parts of the island, as is the case in many of the islands of those seas. Barbadoes is composed of limestone, in great part of fossil madrepores, and traces of organic structure are to be met with in almost every part of the island, more particularly along the whole of the S. and S. W. coast. The land, which when seen from the sea, appears to rise uniformly from the coast, is observed on a nearer view, to consist of successive terraces rising in two or three gradations, one above the other, each forming a plain of a quarter or half a mile in breadth, and terminated

by

by a cliff of coral rock varying in elevation from 12 to 20 feet, and sometimes considerably higher. Deep fissures have, in many places of the island, rent asunder the cliff; and these gullies, as they are called, are continued across the terraces in irregular lines. Numerous caves are every where to be met with, and these are sometimes of very large dimensions. On the S. and S. W. side of the island may be seen at very low water a bed of calcareous sandstone, dipping S. W. 30°. To the eastward of the garrison of St. Ann's is found a dull compact chalky looking limestone, with ramose alcyonia; while considerably to the westward the rock is more distinctly coralloidal. Upon the N. and N. E. side of the island is a small mountainous district called Scotland; consisting almost entirely of limestone, but of a kind less marked by organic remains than in the other districts. In Mr. Parkinson's note it is observed, that some of Dr. Skey's specimens illustrated the nature of some fossil corals; showing, that the forms, in which they at present exist, are not those which belonged to these substances in their original state; and consequently ought not to affect their specific or generic distinctions.

Tubes in drifted sand.

A letter from E. L. Irton, Esq. describing some remarkable tubes found in the drifted sand at Drigg in Lancashire, was read; together with an account by W. H. Pepys, Esq. Treas. G. S., of a chemical examination, made by him, of the substance of these tubes. These tubes are found nearly in a perpendicular position, imbedded in the midst of the hills of drifted sand on the seashore, without any communication with the surface; there are ramifications extending from them, which generally point downwards, and terminate in fine points. The tube sent to the society is above an inch in diameter and of an irregular form. The outside consists of black and white sand, agglutinated together, the inside is smooth, and has a vitrified appearance. When dug out of the sand it was soft, and in some degree flexible, and the inside coating at its first exposure to the air was soft to the touch, and rather unctuous, but in less than a quarter of an hour it hardened into the state in which it now exists. The tube, when found, was filled with the sand of the hill, and that sand is quite different from the sand

sand of which the outside of the tube consists. Both the sand and the vitreous part of the tube scratch glass; and on the latter, when viewed by a lens, there are seen small air blebs, such as are common to imperfect vitrification. Both are insoluble in sulphuric and nitric acids; infusible before the blowpipe without addition; partially fusible on the addition of boracic acid but; with soda a complete fusion took place, and the residue was nearly soluble in water.

A paper by Dr. Mac Culloch, M. G. S. on the vitrified fort of Dun Mac Sniochain, near Oban in Argyleshire, was read. In the discussion which some time ago took place respecting the vitrified forts of Scotland, the question on which the two contending parties were most at issue was, whether the vitrification was the effect of design or of accident. It occurred to Dr. M., that light might be thrown on the subject by examining with mineralogical accuracy the substances of which these structures were composed; and noting the changes, which each had undergone, in consequence of the fire; and also by observing whence the stones had been derived, which were used in them. And that the question of accident or design might be illustrated, by examining in the laboratory the degree of heat required to produce the appearances in the stones, which actually exist in these structures.

Vitrified forts in Scotland.

The fort of Dun Mac Sniochain stands on a long narrow hill, which is nearly precipitous along three parts of its circumference; and at the other end it rises from the plain with a very accessible acclivity. The walls, which are nearly all at present buried under the soil, are about eight or ten feet in thickness. They bear marks of vitrification through their whole extent, but in no case does it appear to have extended more than a foot or two upwards, and the most perfect slags are found at the bottom of the foundation. In the higher parts there are stones roasted by the action of the heat, but unvitrified; and at length the marks of fire almost entirely disappear. The hill consists of alternate beds of schistus and limestone, but the latter is the predominant rock. It is perfectly insulated in a great alluvial plain. The mountains of Benediraloch, which bound the plain to the west, consist of granite, gneiss, mica-slate, quartz

Fort of Dun Mac Sniochain

quartz and porphyry. On the edge of these rocks are found large detached masses of puddingstone, consisting of rounded pebbles of greenstone of different varieties, of amygdaloid and quartz cemented by a paste, which appears to consist chiefly of trapsand united by the hard variety of calcareous spar. The paste contains also, in small quantity, zeolite, prehnite, garnet, and diallage. This puddingstone, where nearest to the fort, is at least half a mile distant from it. The walls of the fort consist principally of granite, gneiss, mica-slate, clay-slate, quartz, puddingstone, and pyritical slate, entangled together; with a very small proportion of the particular rock on which the fort itself is founded: the puddingstone forming the greater part of them. This puddingstone Dr. M. shows to be the only vitrifiable ingredient of the walls; and from the distance from which it must have been brought, and the great quantity of it employed in the work, he considers it probable, that the builders of the fort must have been acquainted with its vitrifiable nature, and that it was on account of this quality, that they had employed so great labour in transporting it. For if their object had not been to produce vitrification, but merely to erect a dry wall of stone, the limestone of the hill would have answered their intentions, or perhaps the loose stones of the adjoining plain. That they did not obtain the puddingstone from the latter source is evident; for although the plain and shore are covered with fragments, these consist almost entirely of the primary rocks; and besides, the pieces of the wall which have not felt the fire are angular fragments, showing pretty clearly, that they were not collected on an alluvial plain, but broken from the rocks where they are found. Dr. M. next proceeds to describe the various states in which the different stones are found. The puddingstone exhibits the greatest variety of changes, it is found in every state, from a black glass to a spongy scoria capable of floating in water, sometimes exhibiting the gradual succession of changes from incipient calcination to complete fusion. To ascertain the degree of heat necessary to produce the corresponding changes in this rock, Dr. M. submitted various parts of it to the furnace, and he found, that some of the fused substances must have been brought to

The vitrification probably the effect of design.

to that state in a heat not less than 100° of Wedgwood's scale; a heat at which many varieties of earthenware are baked. Dr. M. next gives a short account of the vitrified fort of Craig Phadric in Inverness-shire, and of another in Galloway; in both of which, but more particularly in the former, he observed circumstances quite analogous to what he had already found at Dun Mac Sniochain: and the conclusion he has been led to form is, that the vitrification of these forts is the effect of design.

The Society adjourned till November.

Horticultural Society.

It has been the intention of the Horticultural Society, from its first institution, to present annually honorary premiums, or medals, to such persons as have raised, and produced before them, any new and valuable variety of fruit, or esculent plant, or who have made any important discovery in horticulture. But as the Society conceived every one of these to be still capable of acquiring a greater degree of perfection than it has yet attained, they did not think it necessary to direct the attention of gardeners to the improvement of any particular plant. Subsequently, however, they have been induced to think, that it might be advantageous, to publish an account of such projected improvements as shall be suggested by their members, or others, and approved by their council; and the following are therefore proposed, as objects deserving, among others, the attention of experimental horticulturists.

Objects for which the society intend to present premiums and medals.

New varieties of the potato, better calculated for forcing, and for supplying the markets early in the summer, than those at present cultivated.

Other varieties of the same plant, which will afford abundant crops, and be capable of being longer preserved in perfection than any now known, so that the markets might always afford the potato, as nearly as possible in the greatest state of perfection.

A rich and sweet variety of the common red currant, which might probably be obtained from seeds, by appropriate selection, through a few successive generations.

New

Objects for which the society intend to present premiums and medals.

New varieties of the gooseberry, which might supply the markets with green fruit at earlier periods, and mature fruit at earlier and later periods than those now cultivated.

New varieties of pears, similar to those which have been introduced from France; but sufficiently hardy to grow and ripen on standard trees, and calculated to supply the markets at a moderate price during winter and spring.

A good and early new variety of grape, better adapted to the climate of Great Britain, in the open air, than any now known.

Better and more productive varieties of the apple, and capable of being longer preserved in perfection, than most hitherto known.

A good early nectarine; a variety of the strawberry earlier than the common scarlet; and of the cherry, which would ripen before the early may.

More early and hardier varieties of the peach, which might succeed better, at least, than any now known, on standard or espalier trees.

Several native varieties of the plum afford blossoms so hardy, that they are rarely injured by frost. Might not rich varieties be obtained by introducing the farina of the fine but tender kinds into the prepared blossoms of these? It is stated, in the Pomona Herefordiensis, that very rich and very hardy varieties of the apple have been thus obtained immediately from the seeds of the Siberian crab.

In pointing out the preceding objects, as deserving the attention of gardeners, it is not the intention of the society to limit its patronage to those solely: on the contrary, it is their wish, to promote and encourage successful experiments, in every branch of useful and ornamental horticulture.

To Correspondents.

The communications from Mrs. Ibbetson and E. G. shall be inserted the earliest opportunity.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXXII.

ARTICLE I.

Observations on the Disease in the Potato, generally called the Curl; pointing out the most probable Method of preventing it; with an Account of the Results of a few Experiments made on the Subject. By Mr. THOMAS DICKSON, *Leith Walk, Edinburgh* *.

THIS disease, so far as I can learn, first began to be alarming to the growers of the potato about thirty-five or forty years ago. Since that time, it has continued to engage the attention of many eminent agriculturists and gardeners. Date of the curl in potatoes.

Various opinions have at different times been advanced as to its cause. Some were of opinion, that the disease was caused by the tubers used for seed-stock not having been sufficiently ripened:—others thought, that they had been frost-bitten, in the course of the preceding winter:—some ascribed the evil to the effects of blights attacking the plants in coming through the ground;—others to the attacks of certain minute insects:—lastly, the exhausted state of the soil was blamed for the disease. But no one seems to have hit upon the real cause, until the honourable Baron Hepburn of Smeaton, in East Lothian, one of Opinions respecting its cause. Real cause.

* Memoirs of the Caledonian Horticultural Society, vol. I, p. 49.

the most successful and intelligent agriculturists of this country, started a new theory on the subject; which, from its singularity, and *seeming* inconsistence with our experience in matters of a similar nature, did not at the time meet with that attention, to which it undoubtedly was entitled. The Baron thought, that the curl was probably caused by the tubers used for seed-stock having been allowed to become too ripe the preceding year; and that this practice of overripening, being repeated year after year, was the real cause of the disease, the vegetative power in the tubers being thus exhausted.

Experiments made with a view to its proof.

I candidly confess myself to have been rather at first a sceptic on the subject; but, after considering the thing a little, my doubts began to clear away. In order to satisfy myself thoroughly, I resolved upon making a suite of experiments. I accordingly did so; and as they were conducted entirely by myself, or under my own immediate superintendence, I can pledge myself for their accuracy. I now beg leave to lay them before the Caledonian Horticultural Society, in hopes that they may, by means of the Society, be made known to the public; and as the experiments are easily repeated, that they may induce others to turn their attention to the subject.

I think it right to observe, that the experiments now to be detailed were not made with any view of their ever appearing before the public; nor would they have been brought forward at this time, but from a wish to promote the views of this Society.

Propagation of particular varieties by cuts.

It is well known to all cultivators of the potato, that the usual mode of reproducing any particular variety of this valuable root is by cuts or sets of the tubers; and that this mode of propagation is repeated every year, so long as that particular sort is wished for, without our ever thinking of reinvigorating the *seed-stock* *, by raising new plants from the real seeds. In this way it happens, that merely the individual *variety* is propagated; the *species* being reproduced only by sowing the true seeds of the plant. It

* By this expression is always to be understood the stock of *tubers* for planting, in contradistinction to the real seed of the plant.

is only by sowing the seeds that we obtain *new* varieties. But if the seeds be taken from any particular variety, that is wished to be preserved, and if care be exercised, that the plants shall have no communication with the farina of any other plants of the same species in flower, then the produce of these seeds will probably be the same, or nearly the same, with that variety from which the seeds were saved; and from the seed-stock being renewed and reinvigorated in this way, it seems likely, that the variety so obtained may, by observing a proper management, be preserved from the curl or any other kind of degeneracy, for any length of time.

May be effected by seeds with care.

I shall presume, that the principal cause of the curl in the potato is the overripening of the seed-stock for the supply of the ensuing year, by allowing it to remain too long in the ground, and especially if it be also planted early; this practice, being repeated for several years successively, causes an exhaustion of the vegetative principle in the tubers, which renders them totally unfit to produce vigorous healthy plants; and is the principal cause of the disease. This doctrine has almost uniformly been objected to by many very intelligent agriculturists and gardeners, as being quite contrary to our experience in regard to seeds in general; full ripeness being considered the best recommendation. But this objection, I apprehend, arises from the taking an improper view of the subject. It is true, that all of what are properly called *seeds* are improved, by being thoroughly ripened; but *cuts* or *sets*, taken from the tubers of a potato, cannot, strictly speaking, come under the description of seeds. Planting cuts of the potato is analogous to budding or grafting of trees, being only a secondary mode of propagation; and, consequently, the above-mentioned objection does not hold good. This doctrine may be farther illustrated, by observing the strong tendency, which potatoes raised from seeds have to run to flower and seed, unless prevented, by destroying these as they appear, and by earthing up the roots of the plant, so as to induce them to throw out tubers. This natural disposition of plants raised from seeds will remain for several generations of the plant, gradually yielding to the artificial

Curl produced by using over-ripe potatoes for sets.

Objection to this hypothesis

answered.

Growth of potatoes raised from seed.

Argument from a successful practice; means used, until they at last become what we wish. And what may be deemed still a farther proof is, that those who cultivate potatoes most successfully, in the low and early parts of this country, where the disease chiefly exists, bring a supply of seed-stock from the higher and later parts of the country, for a change, every second year at farthest. In such high places, from the lateness and wetness of the climate, the farmers are prevented from planting their potatoes so early as in the low country, and are also, from the fear of early frosts, obliged to take up their crop sooner; consequently the tubers are never so highly ripened as to weaken the vegetative principle in them. Here, then, we have a strong practical testimony to the truth of the docrine which has been advanced.

and the effects of its opposite. On the other hand, in the early districts of the low country, where, as has already been remarked, the disease is principally known, particular kinds of potatoes are planted year after year successively, from the same seed-stock; and most of the early kinds are planted soon in the season, with a view to procure an early crop for the market: a part of these is generally allowed to remain in the ground till the usual time of taking up, to supply seed-stock for the ensuing year: by this time, however, the plants have become so ripe as to weaken very much the vegetative power of the tubers. This practice being repeated for several years, at last so impairs the vegetative power in the tubers, as to produce the curl; and there is no doubt, that, if this practice were persevered in, it would ultimately destroy the power of vegetation altogether, as I have proved by experiments.

Potatoes to be used for setting should never be suffered to flower. There is yet another powerful cause, which weakens the vegetative power in the tubers; and this is, the allowing such plants as are intended to supply seed-stock for the ensuing year, to run to flower, and produce seed*. This should in all cases be prevented, by cutting off the flowers as they appear, even in embryo. Thus, by turning nature from her ordinary course, we force her to exert herself in

* It is generally the late sorts of potatoes, that produce seeds, very few of the early kinds doing so.

another

another channel, and to throw back into the tubers that portion of the vital principle of the plant, which would have been exhausted in the formation of flowers and seeds. Nothing will more contribute to prevent degeneracy in the potato, and especially to prevent curl, than this treatment.

Experiments by the author.

In proof of what I have already advanced on this subject, I shall now state a few experiments made by myself in the years 1801,-2,-3. They appear to me to be quite conclusive, and will go farther to convince, than a volume written without experiments.

In the autumn of 1800, when in Fife, at a friend's house, I met with a potato of the long flat kind *, which I thought very excellent, and obtained a few to cultivate for my own use: he however informed me, that they had been so infested with the curl for some years, that he had resolved to abandon the culture of them altogether. This led me to conclude, that, from their shape, &c., they were well adapted for being made the subject of some experiments I had previously resolved to make, with a view to ascertain the truth of the new idea, upon the cause of the curl, which had been some time before mentioned to me. Accordingly, I selected about half a peck (14lb.) of these, as near the size and shape of the annexed sketch as possible. I took one or two sets from each end of each potato, that is, from the extreme, or dry end, and from the umbilical, or wet end, next the connecting radicle: each sort was planted upon the same ground, but in different rows, with the same kind and quantity of manure to each, and in every respect in exactly the same circumstances, on the 27th of April, 1801.

Sets taken from the opposite ends of potatoes.

Results.

The season was very favourable. Upon examining the plants about the end of June, I found, that all those that were taken from the wet, or least ripened end of the potato, had come up, and were looking well and healthy, except three plants, which were a little affected with the disease: these I threw out, preserving only such as were quite free from it. Upon examining those plants, which were

* A sketch of a tuber of this kind, of the natural size, accompanies this, showing the different cuts or sets, &c.

produced

produced from the dry or ripest end of the potato, I found, that but few of them had appeared above ground, and such as had were all diseased, more or less; but in many instances, the sets had not vegetated at all, nor did they, upon taking them up to examine them, show any signs of vegetation; although quite sound and fresh, they were quite inert; nor did these change their appearance throughout the season, being nearly as fresh when the rest of the crop was lifted, as when they were put into the ground.

On the 30th of July, the whole were again examined; the plants from the unripe sets were almost covering the ground, though planted at two feet between the rows, and were looking well, remarkably free from curl, and promising an abundant crop; while those from the ripened sets, which had vegetated, and had grown, had made very little progress indeed, and were universally curled; several of the plants died after coming a certain length, seemingly from mere weakness; and such as grew stronger had very few tubers at their roots, and these very small and puny.

On the 3rd of October, I took up the produce of both sorts, and pitted them, for renewing the experiment the ensuing year.

The experiment twice repeated.

The same course of experiment was accordingly repeated, not only next year (1802), but also the following year (1803); and the results were exactly similar; the plants produced from the wet, or unripened ends, continuing healthy, and producing abundant crops, while those produced from the dry ends continued to degenerate.

I thus satisfied myself, that the disease originated entirely in the overripening of the seed-stock; and indeed all my experience, since these trials were made, has tended only to strengthen this opinion. I might follow out this to a much greater length, and supply many more facts, all calculated to prove the truth of what has already been advanced; but, by doing so, I should only multiply the detail of similar trials and facts, which, instead of inducing individuals, might rather deter them from satisfying themselves by making experiments. This I should wish them to do.

It

It may be proper to observe, that the produce of the curled potatoes was taken up before being too ripe, and replanted with the others: I cannot say that the disease was removed, but they did not get worse. Perhaps replanting them in very highly manured land, for several years, might have a good effect: but unless it were for the sake of reclaiming a favourite variety, the experiment is hardly worth making.

Perhaps the curl might be removed by taking up the tubers early.

Having trespassed so long on the attention of the Society, I shall only beg leave to suggest a few simple rules, which, if attended to, will, I am humbly confident, soon entirely banish the disease of curl from the country. These are,

Means of preventing the curl.

1. To procure a sound healthy seed-stock, which cannot be relied on, unless obtained from a part of the high country, where, from the climate and other circumstances, the tubers are never overripened.

2. To plant such potatoes as are intended to supply seed-stock for the ensuing season at least a fortnight later than those planted for crop, and to take them up whenever the *haulm* or stems become of a yellow-green colour: at this period, the cuticle or outer skin of the tubers may be easily rubbed off between the finger and thumb.

3. To prevent those plants, that are intended to produce seed-stock for the ensuing year, from producing flowers or seeds, by cutting them off in embryo; taking care, however, to take no more off than the extreme tops, as, by taking more, the crop may be injured. The best mode of doing this, is with a common reaping-hook, or light switching bill. Two boys or girls may do an English acre in two or three days.

Nurseries, Leith Walk,
6th of March, 1810.

References to Plate VIII, *Fig.* 1.

A The ripened or dry end.
B The waxy or wet end.
a a The cuts or sets from the dry end.
b b The cuts or sets from the umbilical end.
c c The umbilical cord or connecting radicle.
d d The real roots of the plant.

II. *Electric*

II.

Electric Attractions and Repulsions are not explained in a Satisfactory Manner in the Hypothesis of Two Fluids. By J. C. DELAMETHERIE *.

Electrical attractions and repulsions said to be easily explained on the supposition of two fluids.

" ELECTRICAL attractions and repulsions," says the author † of the Elementary Treatise on Physics, vol. I, p. 590, 2d ed., " form one of those subjects, that have most engaged the attention of philosophers, and have most embarrassed those, who have endeavoured to refer to the action of a single fluid two diametrically opposite effects, which frequently succeed each other very rapidly in the same body. But if we admit the combined action of two fluids, the theory acquires such a happy simplicity, that the simple enunciation of the hypothesis seems to be a concise explanation of the phenomena.

" Mutual repulsion of two bodies, the electricities of which are homogeneal.

Instance in repulsion.

" § 557. If we suppose in the first place two bodies, each electrified by an additional portion of vitreous or resinous electricity, that has been transmitted to it, we see instantly what must take place; since this principle, that bodies animated with the same kind of electricity repel each other, and that bodies solicited by different electricities attract each other, is only as it were a literal translation of that other fundamental principle, *that the particles of each of the component fluids act on one another by repellent forces, and exert attractive forces on the particles of the other fluid.*

This explained experimentally.

" § 558. This however requires some details, which will find their place in the exposition we are about to give of the means, that may be employed to prove this principle by experiment. Let A B, *Pl.* VIII, *Fig.* 2, be two balls of pith of elder, or any other conducting matter, suspended by threads at a small distance from each other, and to which the vitreous electricity has been communicated. The fluids surrounding these balls mutually repel each other;

* Journ. de Phys. vol. LXV, p. 315.

† Mr. Haüy.

and

and their particles would be diffused through space by opposite movements, if the surrounding air did not retain them near each body. Hence they can only glide on the surface of the body, so, for instance, that the fluid of the body A, being crowded toward the posterior part of this body, *d*, will exert its effort on the air itself, that is adjacent to this part. Thus the equilibrium between this air and that contiguous to the anterior part, *c*, being broken, *the latter will act by its elasticity on the body* A, *to impel it in the direction c h*. The same reasoning applies in the opposite direction to the body B; whence we conclude, that the fluids and the bodies, or balls, impelled by a common movement, must recede from each other. We should have a similar result, supposing the two bodies to be electrified resinously.

" Mutual attraction of two bodies, the electricities of which are heterogeneous.

" § 559. Let us imagine, that, one of the two bodies, A for instance, being solicited by the vitreous electricity, the electricity of the other, B, is resinous. The fluids then will attract each other; so that, with respect to the body A, which we shall continue to take as the object of comparison, the crowding will take place toward the anterior part of the body, *c*. The fluid accumulated in this place then will act repellently on the neighbouring air: whence it follows, *that the air contiguous to the posterior part, d, will impel the body in the direction d n*. The same effect will take place in an opposite direction with respect to the body B, and thus the fluids and the bodies will be carried toward each other."

Instance in attraction.

What has been said is copied literally, that the author's opinion might not be misrepresented.

This explanation of electric attractions and repulsions by the action of two fluids does not appear to me satisfactory. I conceive it may be refuted by a single experiment. The two experiments related by the author, § § 558 and 559, succeed as well in the vacuum of an airpump, as in the open air: consequently neither the attraction nor the repulsion of the two little balls is produced by the action of the atmospheric air.

Proof that this explanation is erroneous:

In

and that the contrary should take place if the supposition were true.

In the next place I would observe, that, if we suppose the air to be driven back by the electric fluid gliding over the ball, the effect should be the reverse of what takes place: for, as this electric fluid acts with sufficient force against the air to drive it back, and the little ball is very moveable, the same thing should take place as with an eolipile, sky-rocket, &c. The sky-rocket, for example, ascends in the air only because the powder as it burns makes a continual jet, which strikes and drives back the air with great velocity. The air resists this rapid movement; and the rocket, being movable is driven forward, and proceeds with more or less rapidity. This is also the cause of the recoil of cannons, muskets, &c.

III.

Memoir on the Proportion the Evaporation of Water bears to the Humidity of the Air. By HONORE FLAUGERGUES*.

Humidity of the air influences evaporation

BY the humidity of the air should be understood the proportion, that the quantity of water mingled and suspended in a given quantity of air bears to the quantity of the air. The more humid the air, the slower and less considerable the evaporation: and there is even a degree of humidity, at which evaporation wholly ceases, because, the air being loaded with all the moisture it can contain, no more can rise. I have made a great number of experiments, to ascertain the law, that this decrease of evaporation follows; and as experiments of this kind appeared to me most proper to determine the general law of evaporation as it respects the moisture of the air, I shall confine myself here to those, on the results of which most dependance can be placed, as I employed in making them an extensive apparatus, of which the following is a description.

Experiments to ascertain the law of its action.

Apparatus described.

I began with procuring a stock of air completely dried for the purpose in a very dry season. I caused a cask to be hooped and headed, that would contain about nine cubic feet,

* Journ. de Physique, vol. LXX, p. 157.

feet, very clean, and the staves and heads of which were perfectly dry. I luted the joints accurately; and for greater precaution pasted over them slips of paper, to prevent all access of the external air. Into the bung-hole of this cask I poured about two cubic feet of quicklime coarsely powdered, and hot from the kiln. The bung-hole was then closed with a very tight straight cock, the key of which was perforated with a hole about three lines in diameter. In this state I left things about three weeks, shaking the cask several times a day, that the lime might present fresh surfaces to the contact of the air, and thus more speedily and completely free it from all the water, that might be suspended in it. Thus presuming the air to be perfectly dried, I employed it in my experiments, pouring it into a tin vessel, which I had made for the purpose.

Preparation of the air.

This vessel consists of a hollow cylinder, 13 in. 2 lines * in diameter, and 18 inches high. This cylinder is closed at one end by a circular plane, and at the other by a cone 3 in. 7 lines high. Its capacity is about 2614 cub. in. The summit of the cone is truncated; and has soldered to it a small cylindrical tube, capable of receiving a cylindrical vessel of glass, four lines in diameter, intended to hold the water to be evaporated into the air in the vessel. This glass is fixed in the tube by means either of putty or of soft wax. The tin vessel may be fixed in a perpendicular position, with the glass vessel downwards, by means of two handles soldered to the cylindrical part, and resting on two supports of iron fixed upright on a table. The sheets of tin, of which this instrument is made, are very carefully soldered; and the external air cannot find admittance, except through the aperture of the tube at the summit of the cone when this is not closed by the glass vessel. To fill this vessel with the dry air in the cask, I began by filling it completely with fine sand, perfectly dried by a fire. I then placed it on the cask, so that the aperture at the end of the conical part was exactly fitted to the cock; and, after carefully closing the juncture with soft wax, I turned the key of the cock. The sand, escaping by the hole in

Vessel for making the experiments.

Mode of filling the vessel.

* The French measures are here retained. C.

the

the key, ran into the cask, and the air displaced by it at the same time ascended into the vessel. When the sand, by the help of a few slight shakes, had entirely run out, and the vessel was filled with the dry air of the cask, I shut the cock, carefully removed the tin vessel, and immediately introduced into the tube the orifice of the cylindrical glass, filled to within three lines of the top with very pure rain water; and cemented it there so that it could not fall, and that none of the external air could get in. I then placed this vessel, keeping it always upright in the way I have mentioned, in a room, where I kept up a uniform temperature during each series of experiments. The glass cylinder at the bottom of the vessel being thus completely isolated before the window of the room, it was easy to measure the sinking of the water in it by evaporation, by taking with a pair of very pointed spring compasses, and with the assistance of a good lens, the distance from the surface of the water to the level at which it stood at the commencement of the experiment, this being marked on the glass with a diamond. For calculating this distance I employed the same scale of a thousand parts, made by Canivet, as I used for my experiments on the relation between heat and spontaneous evaporation*. Every day at the same hour, four o'clock mean time, I took the measure of the fall of the water below its original level: but in the following table, to save room, I shall set down only the measures of every third day, confining myself also to the four series of experiments that succeeded best.

Manner of making the experiments.

Heat and pressure during the experiments.

During the first of these series, the thermometer by the side of the tin vessel was constantly at 20° [77° F.]; and the height of the barometer, when the vessel was filled with the air, was 27 in. 9·7 lines [29·64 in. Eng.].

In the 2nd series the thermometer continued with very little variation at 18° [72·5° F.]; and the height of the barometer at the commencement was 28 in. 0·4 l. [29·88 in.].

During the 3rd series the thermometer marked nearly 15° [65·75° F.]; and the height of the barometer 27 in. 8·4 l. [29·52 in.].

* J. de Phys. vol. LXV, p. 446: or Journal, vol. XXVII, p. 17. On this scale 190 parts were equal to a Paris inch.

In

In the 4th series the thermometer was constantly at 10° [54·5° F.]; and the barometer at the beginning was at 28 in. 2·9 l. [30·1 in.].

The results of these experiments are given in the following table, divided into four columns. The first of these contains the date; the second, the distance below the level at the commencement; the third, the distance fallen between the times of measuring. These differences form apparently a decreasing geometrical progression, as I quickly perceived: but to render this law more evident I have added a fourth column, in which the differences are calculated by inserting five mean geometrical proportionals between the first and last difference found by experiment. The table explained.

Tabulated Results.

Date of the measures. 1807.	Fall of the water.	Actual differences.	Differences calculated.	Date of the measures. 1807.	Fall of the water.	Actual differences.	Differences calculated.
First Series.				Third Series.			
Aug. 1	Parts 0			Sept. 18	Parts 0		
4	59	59	59	21	35	35	35
7	96	37	35·9	24	55	20	21·7
10	117	21	21·9	27	70	15	13·5
13	132	15	13·3	30	78	8	8·4
16	140	8	8·1	Oct. 3	82	4	5·2
19	145	5	4·9	6	86	4	3·2
22	148	3	3	9	88	2	2
Second Series.				Fourth Series.			
Aug. 24	Parts 0			Oct. 12	Parts 0		
27	47	47	47	15	24	24	24
30	73	26	27·8	18	37	13	14·1
Sept. 2	91	18	16·4	21	44	7	8·3
5	102	11	9·7	24	50	6	4·9
8	108	6	5·7	27	53	3	2·9
11	111	3	3·4	30	54	1	1·7
14	113	2	2	Nov. 2	55	1	1

As

The evaporation decreased in geometrical progression.

As the vessel containing the water evaporated in the preceding experiments was cylindrical, the successive diminutions of the height of the water are proportional to the quantities evaporated; and as these successive diminutions form a decreasing geometrical progression, as may easily be verified, we shall conclude, that the quantities of water evaporated in equal times, in the same body of air, likewise form a decreasing geometrical progression. Hence it is easy to ascertain the law followed in the evaporation of water with respect to the humidity of the air, by means of a very simple geometrical construction.

The law exemplified by a diagram.

Let H I, *Pl.* VIII, *Fig.* 3, be an hyperbola, described between the rectangular assymptotes C A, C K. On the assymptote C A take the abscissas C A, C B, C D, C E, &c., in a decreasing geometrical progression; and, erecting the perpendicular ordinates A H, B F, D G, E I, the hyperbolical spaces A H B F, B F G D, D G E I, will be equal*; and the parts A B, B D, D E, of the assymptote, will be in a continued decreasing geometrical progression: for, since, by construction, C A : C B : : C B : C D : : C D : C E, we shall have *dividendo* C A : C A − C B (A B) : : C B : C B − C D (B D) : : C D : C D − C E (D E); and *convertendo* C A : C B : C D : : A B : B D : D E. We may represent the lowering of the water therefore, or the evaporations, by the lines A B, A D, &c.; and the times of these evaporations by the corresponding hyperbolical spaces A H B F, A H D G, &c. This admitted, let us suppose A C to represent the quantity of water necessary to saturate completely the body of air, in which the evaporation takes place; and the hyperbolical area A H E I to represent any time, taken at pleasure: A E will represent the quantity of water evaporated during that time, and C E the difference between the quantity of water necessary for the complete saturation and the quantity of water evaporated. Farther, if we draw *e i* parallel and infinitely near to the ordinate E I, E *e* will represent the evaporation that takes place during the fluxion of time represented by the

* F. Deschalles Cursus mathematicus, Sect. conic., Lib. I, prop. xli.

elementary

elementary parallelogram E I $e i$: but by the hypothesis the fluxion of time is constant; E e therefore is inversely proportional to E I, which is inversely proportional to C E; and consequently Ee is directly proportional to C E: that is to say, the evaporation is at each instant proportional to the difference between the quantity of water necessary to saturate completely the body of air in which the evaporation takes place, and the quantity of water actually evaporated and suspended in that air; or, in other words, the evaporation is proportional to the excess of the moisture of the air at the point of saturation over the present humidity. This is the general law, which evaporation follows with respect to the humidity of the air.

This law appears to confirm the opinion of Muschembrœck * and Leroy †, that the evaporation of water is nothing but a solution of this fluid in the ambient air; for the law just announced must take place generally in all solutions. In fact, if the menstruum did not exert on the body to be dissolved, in every portion of time, an action proportionate to the quantity that remained to be dissolved to produce complete saturation, but a greater or less action, it would follow, that, in the first case, when the menstruum is completely saturated it would still retain a part of its solvent action, which would remain without effect; and in the second, that the effect would be greater than its cause, which is equally absurd.

Tends to confirm the opinion that evaporation is a true solution.

The application of this law supposes a knowledge of the quantity of water necessary to saturate completely a given quantity of air at a given temperature. To endeavour to accomplish this object, I repeated the elegant experiments of Mr. de Saussure ‡: but not having a large globe, I could only employ glass jars, the apertures of which I closed with a plate of metal cemented all round. Notwithstanding the imperfection of this apparatus, I had the satisfaction to obtain the same result as that celebrated philosopher; namely, that it required about 10 grains of water to saturate completely a cubic foot of air at the temperature

Attempt to find the quantity of water that would saturate air at a given temperature.

* Diss. Phys. Leyden, 1751, vol. II, p. 721.

† Mém. Acad. 1751, p. 484 and foll.

‡ Essais sur l'Hygromètrie, N. 97 and fol.

of

of 15° [65·75° F.]. I repeated the same experiments with air at the temperature of 20° [77° F.] and 5° [43·25° F.], and I found, that it required in the first case about 16·75 grs., and in the second 4·5. These three quantities are nearly in the same ratio as the evaporations under these degrees of heat given in the table inserted in this Journal*; and it is obvious, that it could not be otherwise.

Humidity of saturated air at 32°.

From this principle we have the proportion 17·1 : 4·4 : : 10 grs : 2·6 grs, the weight of the quantity of water contained by a cubic foot of air at the temperature of melting ice, when completely saturated. The weight of a cubic inch of water being 373·5 grs, this is the weight of 12 cub. lines of water; which, divided by the bulk of the air, will give $\frac{12}{(144)^3}$ for the humidity of the air at the temperature of melting ice, when completely saturated with water.

Rule for finding the point of saturation at other temperatures.

By the same reasoning may be found the humidity of a cubic foot of air at the temperature X, and at the point of saturation; by means of the proportion

$$(2{\cdot}718)^{\frac{0}{11\cdot05}}(4{\cdot}4) : (2.718)^{\frac{X}{11\cdot05}}(4.4) : : \frac{12}{(144)^3} : \frac{12{\cdot}(2{\cdot}718)^{\frac{X}{11\cdot05}}}{(144)^3}.$$

This 4th term expresses the humidity to which the evaporation in air perfectly dry, at the temperature X, is proportional.

If we call Z the number of cubic lines of water suspended in a cubic foot of air, the temperature of which is equally X, the humidity of this air will be expressed by $\frac{Z}{(144)^3}$, and the evaporation in this air, agreeably to what has been said, will be proportionate to

$$\frac{12(2{\cdot}781)^{\frac{X}{11\cdot05}}}{(144)^3} - \frac{Z}{(144)^3}.$$

* Journ. de Phys., vol. LXV, p. 451; or our Journ. vol. XXVII, p. 22.

Consequently

Consequently the evaporation in air perfectly dry is to the evaporation in air that contains Z cubic lines of water in the cubic foot, at the same temperature X, in the ratio of $(2{\cdot}718)^{\frac{X}{11{\cdot}05}}$ to $(2{\cdot}718)^{\frac{X}{11{\cdot}05}} - \frac{Z}{12}$.

I have concluded from various experiments, that, by increasing the linear factor of the formula of evaporation * one fifth, this formula would pretty accurately represent the evaporation in air perfectly dry. If we make this correction, and reduce to lines by multiplying by the proportion $\frac{12}{190}$, confining ourselves to two decimal places, this formula will become (A) . . . . $(2{\cdot}72)^{\frac{X}{11{\cdot}05}}$ (0·34 lines); consequently $\left((2{\cdot}72)^{\frac{X}{11{\cdot}05}} - \frac{Z}{12}\right)$ (0·34 lines) is the formula, that gives the value of the evaporation, or lowering of the surface, of water expressed in lines, that takes place in twenty-four hours in air at the temperature of X degrees of De Luc's thermometer, and which contains Z cubic lines of water in the cubic foot. The calculation of the last formula is very simple; since it is sufficient, to deduct from the quantity calculated by the formula (A) the quantity Z (0·03 of a line); to which is reduced the effect of the humidity of the air.

Formula for calculating evaporation at any temperature and humidity of the air.

To facilitate the application of this formula, nothing is requisite but a more convenient and speedy mode of determining the number of cubic lines of water diffused in a cubic foot of air than that of drying this air by potash or quicklime, and then finding the increase in weight of the latter. I have sought this, which would lead, as has been seen, to the discovery of a true hygrometer; but my endeavours have not been more successful, than those of the celebrated natural philosophers, who have paid attention to the same subject.

Desideratum.

The imperfection of these researches has been the reason that I have so long deferred publishing them. I was still in hopes

* Journ. de Phys. vol. LXV, p. 452: or Journ. vol. XXVII, p. 23.

hopes of rendering them more perfect; but not being able to procure the instruments necessary for this purpose, I have resolved to communicate them to the learned, in hopes that these feeble attempts might perhaps induce them to turn their eyes toward this interesting subject, and give us at length a true theory of evaporation. *His principiis via ad majora sternitur* *.

Experiments made in the open air.

I had intended to add to this paper the experiments on a large scale, which I made on evaporation with cylindrical vessels full of water, the apertures of which were from three inches to eleven in diameter, and the height of which varied from eleven inches to eighteen. These vessels were placed in the open air in my garden, and buried within three lines of their apertures, at a little distance from each other. It was from these experiments I inferred evaporation to be proportionate to the surface of the water in contact with the air. I would also publish a journal kept for several months, to compare the evaporation that took place in the open air, and in a large vessel, with my formula, would it not occupy too much valuable room. I mention it, however, to show, that I have not always operated on confined air.

IV.

Remarks on the Construction of Fowlingpieces, pointing out Methods, by which they may be made to throw Shot very close, and the contrary. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

Improvement in the breech of a fowlingpiece.

THE following circumstance led me to take into consideration the construction of fowlingpieces, and to make what I conceive to be a useful improvement in that part, which is called the *breech*. Meeting by chance with an old foreign made gun barrel, of a construction that accorded much with my fancy, I purchased it, and determined to have it fitted up (first having tried it at a mark two or three times).

* Is. Newtoni Tract. de Quadrat. Curvarum, ad calcem.

I then

I then unscrewed the breech, or plug, which closes the hind part of the barrel, and ordered another, the form of which may easily be understood by referring to the annexed representation, Pl. ix, fig. 1. Now, after I had been at considerable trouble, and much pains, for I was determined to have it fitted up under my own inspection, I was very much disappointed in not being able to kill any thing with it, if at a greater distance than about 25 yards, in consequence of the shot being too much scattered. I varied the charge several times to shoot at a mark, but could by no means satisfy myself.

The bottom made concave.

This caused the shot to scatter.

After some time I took out the breech, and filed down the edges of the hollow part to the touch-hole, and fitted it up in the form of a common breech. I now found I had improved the killing quality of my gun, and had got pretty much out of conceit with the concave form of breeching guns. However it had this effect; since, thought I, the form I have just described, impairs the shooting quality of a fowlingpiece, there must be some *contrary* means of improving it; and accordingly I had one made of a form which will be easily understood, by referring to figure 2.

The bottom being made flat, the gun performed better.

Since this last improvement, I can with more certainty, (and I speak within compass) bring down a bird at the distance of about 60 or even 70 yards, than I could when I made use of the breech fig. 1, at the distance of 20 yards.

An improvement in the construction.

E, in fig. 2, is a strong iron or steel peg, standing out of the common breech, up the centre of the barrel, about an inch, (the thickness must be determined by the bore of the barrel), so as to contain the charge of powder round the peg; the wadding of the powder hereby resting upon the top of it, so as to prevent the powder being hard rammed. This not only keeps the grains of powder from being crushed by the ram-rod, but the impulsive force of the newly liberated air, on firing the powder, being removed from the central part of the charge of shot, has an opposite effect to that of the breech, fig. 1, viz. that of concentrating instead of dispersing the shot.

Its effect.

I am, Sir, your humble servant,

Bradford, Yorkshire, July the 7th, 1812. E. G.

V.

Description of an improved Scarificator: by Mr. John Fuller, *No.* 14, *Hatton-Garden* *.

SIR,

Use of the scarificator.

I WISH to submit to the notice of the society an improvement in the construction of the scarificator, which I flatter myself will be found worthy their attention. It may be necessary to premise, that the scarificator is an instrument used in cupping for making the incisions, from which blood is afterward obtained by means of exhausted glasses. As the degree of pain caused by this operation depends on the good or bad quality of the scarificator, this instrument has always been an object of attention. The best scarificators generally in use propel from ten to sixteen lancets, through about half a circle, which is effected by a part, termed the rack, moving on its centre or part of its edge. On part of its edge are situate teeth, which are so confined that by moving the cock or tail of the rack, they work three pinions, and make them revolve about halve a round; it is evident, that lancets fixed on the axis of these pinions must also have a circular motion, and endeavour to cut any thing opposed to their passage; it is likewise evident, that, if a spring be so set, that it can be released, and its force applied on a sudden to the rack, (somewhat similar to the main-spring of a gun-lock) all the lancets will be carried forward at once, and that with a force and velocity in proportion to the strength of the main-spring.

Common construction of it.

Objections to this construction.

The objections to this construction, which frequently occur in practice are, that from the number of lancets necessarily used, the resistance to their motion is so great, that that they do not move with the swiftness requisite to the ease of the patient; it is likewise often desirable to have the incisions rather deep, and then they are often quite stopped in their progress; beside this, by the lancets all moving one way, they are found to drive the skin up in folds, and thus present additional resistance, and occasion excessive pain to the

* Trans. of the Soc. of Arts, vol. xxix, p. 126. The silver medal was voted to Mr. Fuller for this improvement.

the patient; if to obviate these inconveniences the main spring be made very strong, it is then, from its confined situation, exceeding liable to break; and if this does not happen, another inconvenience is produced, viz. from its very great strength it is scarce possible for any person to cock and discharge it with the requisite ease.

Improvement suggested.

It was therefore suggested, that, if two rows of lancets could be made to move in contrary directions, these, by keeping the skin equally stretched, would form clean incisions with much less force than in the former method: The scarificator A, marked No. 1, was therefore constructed, and first used early in 1802; it is accompanied by its working model B; this instrument at first contained the twelve long-edged spiral lancets C.

Defects of this construction.

This instrument immediately showed its theory to be good, but it had its faults; the incisions were too long when of the necessary depth; from the complex nature of the two racks, &c., and from the confined situation to which they were restricted, they could not be placed in the most favourable position for motion, and were therefore liable to be out of order.

These defects removed.

These and many other objections were altered or removed in various ways, which at length terminated in the construction of the instrument D, marked No. 2, also containing twelve lancets, combining every advantage and improvement suggested by experience and reflection. This instrument admits of two main springs, but from the manner in which the racks work in each other, and in their respective pinions, they in effect become one, but maintain the advantage of being made more slight, and consequently admitting a greater extent of motion than a single stiff spring can possibly accomplish; beside which they are not so liable to break; and should this happen to one, the instrument would not be useless, for I believe that one of these springs would be strong enough for all ordinary purposes, as incisions are effected with much less force when the lancets diverge; but combined they never have shown the least disposition to stop, however deep it might be necessary to set them, or strong the integuments to which they were applied; and consequently attended with greater ease to the patient. On

inspecting the instrument and working model E, its simplicity, I flatter myself, will be admitted; nor is any force of the main-springs spent in overcoming unnecessary friction or ill-directed motion; the ease likewise with which the instrument is discharged, consideriug its strength of spring, will be noticed by every one accustomed to scarificators. It may be objected to this instrument that it is larger and heavier than the ordinary scarificator, but it is capable of being reduced without injury to the improvement, as is shown by the instrument F, which is completely within the usual size.

Testimonies in its favour.

I have been favoured with testimonies of approbation from the following respectable gentlemen. Dr. Willan, physician; Mr. Armiger, Mr. Frampton, and Mr. Lawrence, surgeons and teachers of anatomy.—It will be also seen, that, while in the possession of Mr. Armiger, it was approved by the late Dr. Rollo, and by Dr. Irwin, the present surgeon-general and inspector of ordnance hospitals; and I may add as a farther proof of its utility, if more be necessary, that by an illiberal use of private confidence, others have been made on a similar plan; and I am misinformed, if the honour of this invention has not been claimed, within these few months, by three or four different individuals.

Another scarificator to act by puncturing.

Accompanying this is another scarificator, G, the peculiarity of which consists in the lancets being projeced directly forwards, and returning into the box or case; the working model N is not exactly such as is contained in the instrument, but an improvement on it. This instrument and model must be considered merely as experiments, to see how such motion could be effected; for, persuading myself that punctured wounds would be more painful and more difficult to heal in this, as they are usually in other circumstances, compared with incised wounds; and likewise having so happily succeeded in constructing that already described, which I consider so far superior, I never used it, and therefore can say nothing as to its operative merits; nor should I have recalled it from oblivion, but having lately seen a description of a scarificator by punctured wounds, invented by a very ingenious

ingenious and respectable medical gentleman *, who speaks very satisfactorily of its performance, and which does not possess the advantage of the lancets withdrawing from the wounds, they being removed with the instrument, I thought, that should mine be a mistaken opinion, and that some real advantage attends this method, it occurred to me that submitting the instrument to the Society would be applying it to its best use; as from its possessing the property of the lancets withdrawing themselves, it might suggest some ideas for farther improvement.

I am, sir,

Yours obediently,

JOHN FULLER.

Description of the Engravings of Mr. JOHN FULLER's *scarificator, Plate IX.*

Explanation of the plate.

Figs. 3, 4, 5, and 6, are sections of this instrument, taken in different positions, to explain its interior mechanism; fig. 3, is a plan of the lancets, the top of the box being removed to show them; fig. 4, is a section through the centre of the box; and fig. 6, the same, but taken in the opposite direction.

The lancets *a a*, 4, 5, and 6, are fixed upon two small arbors mounted parallel to each other across the box; the lancets are so arranged on the arbors, that those upon one arbor are placed in the intervals between the lancets fixed upon the other; the arbors are placed near the top of the box, and the lancets act through clefts cut in the lid, (as shown in figs. 4, and 6,) when the arbors are turned round; this is performed by a pinion upon each arbor, receiving motion from two toothed sectors, A B, fig. 5, which are also caused to act together by the teeth on their edges; they are fixed upon two parallel spindles C D, which extend across the box; the sector A, has a lever or handle attached to it, and coming through the bottom of the box, and by pulling this, the scarificator is wound up ready for action. The power is given by two horseshoe springs, *e e*, figs. 3 and 6, one end of each is screwed upon the bottom of the box, and the other acts in a notch *f f*, in each sector, so as to press those

* See Journ. vol. xxvii, p. 124.

these sides upwards; the sectors are prevented from moving, except when required, by a catch *g*, which enters notches cut in the handle of the sector A; this catch is a brass bar, lying across the bottom of the box, as shown in figs. 4 and 6, it moves on a screw as a centre pin at one end, and is pressed towards the sector by a slight spring (not seen); when it is to be discharged, a button *h* is pressed in, which disengages the catch *g* from the notches in the piece A, and permits the springs to turn the sectors about, and by their teeth acting in the pinions, turn the lancets round at the same time, which is effected by the teeth of the two sectors engaging each other, in order that they may not operate so as to move the whole instrument upon the skin, as is the case in the common scarificator.

Fig. 5 shows, that the handle of the sector A has two notches in it, for the catch *g:* when it is caught upon the first of these, the instrument is in the position of fig. 4, which is the half-cock, the lancets standing directly upright out of the box; in this position, the depth they are intended to penetrate is adjusted by means of a screw *k*, passing through the bottom of the box; it is tapped into a piece of brass *l*, which is screwed to the lid *m* of the box; the piece of brass is bent, as shown in fig. 6, that it may clear the lancets and their spindles; when the screw is turned, it is evident that it will raise or lower the lid of the box, and cause the lancets to protrude more or less through it, and consequently enter a greater or less depth into the skin.

Method of using the instrument.

When the instrument is to be used, the handle of the sector A is to be drawn back into the position of *fig.* 5, which is at the full cock; the lancets are now turned down wholly within the box, and the springs wound up; the lid is then to be applied flat upon the raised-up skin of the part to be scarified; and by pushing in the button *h*, the catch *g* is moved round on its centre pin, and pushed out of the notches in the handle of the sector A; the springs now turn the sectors, and the lancets fly out of the box with inconceivable rapidity; and making as many punctures in the skin, return into the box, having made exactly half a turn with their respective arbors. The dimensions of the instrument may be ascertained by the scale of inches annexed to the drawing.

VI.

On a Case of nervous Affection cured by Pressure of the Carotids; with some physiological Remarks. By C. H. PARRY, *M. D. F. R. S.* *

Laws of animal life the most important in physics.

OBSERVING that the Royal Society, of which I have the honour to be a member, occasionally receives communications illustrative of the laws of animal life, which are indeed the most important branch of physics, I take the liberty of calling their attention to a case, confirming a principle which I long ago published, and which I believe had never till then been remarked by pathologists.

Nervous affections suspended by compressing the carotid arteries.

About the year 1786, I began to attend a young lady, who laboured under repeated and violent attacks, either of head-ach, vertigo, mania, dyspnœa, convulsions, or other symptoms, usually denominated nervous. This case I described at large to the Medical Society of London, who published it in their Memoirs, in the year 1788. Long meditation on the circumstances of the case led me to conclude, that all the symptoms arose from a violent impulse of blood into the vessels of the brain; whence I inferred, that as the chief canals conveying this blood were the carotid arteries, it might perhaps be possible to intercept a considerable part of it so impelled, and thus remove those symptoms, which were the supposed effect of that inordinate influx. With this view, I compressed with my thumb one or both carotids, and uniformly found all the symptoms removed by that process. Those circumstances of rapidity or intensity of thought, which constituted delirium, immediately ceased, and gave place to other trains of a healthy kind; head-ach and vertigo were removed, and a stop was put to convulsions, which the united strength of three or four attendants had before been insufficient to counteract.

That this extraordinary effect was not that of mere pressure, operating as a sort of counteracting stimulus, was evident: for the salutary effect was exactly proportioned to the actual pressure of the carotid itself, and did not take place at all, if, in consequence of a wrong direction, either

* Phil. Trans. for 1811, p. 89.

to

to the right or left, the carotid escaped the effects of the operation.

Mode in which it acts.

This view of the order of phenomena was, in reality, very conformable to the known laws of the animal œconomy. It is admitted, that a certain momentum of the circulating blood in the brain is necessary to the due performance of the functions of that organ. Reduce the momentum, and you not only impair those functions, but, if the reduction go to a certain degree, you bring on syncope, in which they are for a time suspended. On the other hand, in nervous affections, the sensibility and other functions of the brain are unduly increased; and what can be more natural than to attribute this effect to the contrary cause, or excessive momentum in the vessels of the brain? If, however, this analogical reasoning has any force in ascertaining the principle, I must acknowledge, that it did not occur to me till twenty years afterward, when a great number of direct experiments had appeared to me clearly to demonstrate the fact.

From various cases of this kind, I beg leave to select one which occurred to me in the month of January, 1805.

Case of nervous affection.

Mrs. T. aged 51, two years and a half beyond a certain critical period of female life, a widow, mother of two children, thin, and of a middle size, had been habitually free from gout, rheumatism, hæmorrhoids, eruptions, and all other disorders, except those usually called nervous, and occasional colds; one of which, about two years and a half before, had been accompanied with considerable cough, and had still left some shortness of breathing, affecting her only when she used strong muscular exertion, as in walking up stairs, or up hill.

In February 1803, after sitting for a considerable time in a room without a fire, in very severe weather, she was so much chilled as to feel, according to her own expression, "as if her blood within was cold." In order to warm herself, she walked briskly for a considerable time about the house, but ineffectually. The coldness continued for several hours, during which she was seized with a numbness or sleepiness of her left side, together with a momentary deafness, but no privation or hebetude of the other senses, or pain or giddiness of the head. After the deafness had subsided,

sided, she became preternaturally sensible to sound in the ear of the affected side, and felt a sort of rushing or tingling in the fingers of the left hand, which led her to conclude, that "the blood went too forcibly there." Case of nervous affection.

Though the coldness went off, what she called numbness still continued, but without the least diminution of the power of motion in the side affected. In about six weeks, the numbness extended itself to the right side.

Among various ineffectual remedies for these complaints, blisters were applied to the back, and the inside of the left arm above the elbow. The former drew well. The latter inflamed without discharging: so that a poultice of bread and milk was put on the blistered part. After this period, the muscles of the humerus began to feel as if contracted and stiff: and these sensations gradually spread themselves to the neck and head, and all across the body, so as to make it uncomfortable for her to lie on either side, though there was no inability of motion.

She now began to be affected with violent occasional flushings of her face and head, which occurred even while her feet and legs were cold, together with a rushing noise in the back of the head, especially in hot weather, or from any of those causes, which usually produce the feelings of heat.

It is difficult to give intelligible names to sensations of a new and uncommon kind. That, which this lady denominated numbness, diminished neither the motion nor the sensibility of the parts affected. It was more a perception of tightness and constriction, in which the susceptibility of feeling in the parts was in fact increased; and the skin of the extremities was so tender, that the cold air produced a sense of uneasiness, the finest flannel or worsted felt disagreeably coarse, and the attempt to stick a pin with her fingers caused intolerable pain.

In the month of September 1803, not long after the application of the blisters, she experienced in certain parts of the left arm and thigh that sensation of twitching, which is vulgarly called the "life blood," and which soon extended itself to the right side. Shortly afterward, she began to perceive an actual vibration or starting up of certain portions

Case of nervous affection.

tions of the flexor muscles of the fore-arm, and of the deltoid on the left side; not so, however, as to move the arm or hand.

This disorder had continued with little variation to the period of my first visit. The vibrations constantly existed while the arm was in the common posture, the fore-arm and hand leaning on the lap. If the arm were stretched strongly downwards, the vibration of the flexors ceased, but those of the deltoid continued. The arm being strongly extended forwards, all ceased; but returned as soon as the muscles were relaxed. The vibrations were of different degrees of frequency, and at pretty regular intervals, usually about 80 in a minute. They were increased in frequency and force by any thing which agitated or heated the patient, and were always worse after dinner than after breakfast. The pulse in the radial artery was 80 in a minute, and rather hard. That in the carotids was very full and strong; and each carotid appeared to be unusually dilated for about half an inch in length, the adjacent portions above and below being much smaller, and of the natural size. I much regret, that I find in my notes of this case no inquiry, whether there was any coincidence between the systoles of the heart, and the muscular vibrations. The patient's feet were usually cold, and her head and face hot. The feeling in her limbs was much as I have above described, except that the sensibility was somewhat less acute than it had been, and she complained of a tightness all over her head, as if it had been bound with a close night-cap. Her sleep was usually sound on first going to bed, but afterward, for the most part, interrupted by dreaming. Bowels generally costive: appetite moderate: no flatulency or indigestion: tongue slightly furred, without thirst: urine variable, but generally pale.

Effect of compressing the carotid artery.

The late Mr. George Crook, surgeon, was present while I made these examinations; and when we afterward conversed together, I remarked to him, that if my theory of the usual cause of spasmodic or nervous affections were well founded, I should probably be able to suppress or restrain these muscular vibrations of the left arm, by compressing the carotid artery on the opposite or right side; while little effect

effect might perhaps be produced, by compressing the carotid of the side affected. The event was exactly conformable to my expectation. Strong pressure on the right carotid uniformly stopped all the vibrations, while that on the left had no apparent influence. I may add, that these experiments were afterward, at my request, repeated on this lady in London by Dr. Baillie, and, as he informed me in a letter, with a similar result.

It is perfectly well known to many of the learned members of this Society, that irritations of the brain, when of moderate force, usually exhibit their effects on the nerves or muscles of the opposite side of the body; and in the case before us, it is difficult to understand how the suspension of these automatic motions could have been produced by this pressure of the opposite carotid, in any other way than by the interruption of the excessive flow of blood through a vessel morbidly dilated; in consequence of which interruption, the undue irritation of the brain was removed, and the muscular fibres permitted to resume their usual state of rest.

Undue impulse of blood on the brain the common cause of spasmodic or nervous affection.

From these and many other similar facts, I am disposed to conclude, that irritation of the brain, from undue impulse of blood, is the common though not the only cause of spasmodic and nervous affections; and I can with the most precise regard to truth add, that a mode of practice, conformable to this principle, has enabled me, during more than twenty years, to cure a vast number of such maladies, which had resisted the usual means.

An investigation of all the modifications of the principle itself, and of its numerous relations to therapeutics, would be inconsistent with the views of the Royal Society, and must be reserved for another place.

Bath, Dec. 8, 1810.

VII. *A concise*

VII.

A concise View of the Theory respecting Vegetation, lately advanced in the Philosophical Transactions, illustrated in the Culture of the Melon. By T. A. Knight, *Esq. F.R.S., &c.**

Theory of vegetation exemplified in the melon.

THE Council of the Horticultural Society having desired that I would send them a general view of my theory on vegetable physiology, which has been published by the Royal Society, I have great pleasure in obeying their wishes; and conceiving, that I shall be able to render it more clear amd useful, by making it illustrative of the proper culture of some particular plant, and by referring the reader to the papers in the Philosophical Transactions for evidence in support of the circumstances stated, I have for this purpose chosen the *melon.*

The seed.

A seed, exclusive of its seed-coats, consists of one or more cotyledons, a plumule or bud, and the caudex or stem of the future plant, which has generally, though erroneously, been called its radicle †. In these organs, but principally in the cotyledons, is deposited as much of the concrete sap of the parent plant, as is sufficient to feed its offspring, till that has attached itself to the soil, and become capable of absorbing and assimilating new matter.

The plumule.

The plumule differs from the buds of the parent plant in possessing a new and independent life, and thence in assuming, in its subsequent growth, different habits from those of the parent plant. The organizable matter, which is given by the parent to the offspring in this case, probably exists in the cotyledons of the seed, in the same state as it exists in the alburnum of trees; and like that, it apparently undergoes considerable changes before it becomes the true circulating fluid of the plant: in some it becomes saccharine, in others acrid and bitter, during germination ‡. In this process the vital fluid is drawn from the cotyledons into the caudex of the plumule or bud, through vessels which cor-

Caudex.

* Hort. Trans. vol. I, p. 217. Journ. vol. XXV, p. 118.

† Phil. Trans. 1809, p. 169:

‡ Phil. Trans. 1805.

respond

respond with those of the bark of the future tree, and are indeed perfect cortical vessels *. From the point of the caudex springs the first root, which, at this period, consists wholly of bark and medulla, without any alburnous or woody matter; and, if uninterrupted by any opposing body, it descends in a straight line towards the centre of the Earth, in whatever position the seed has been placed, provided it has been permitted to vegetate at rest †. First root.

Soon after the first root has been emitted, the caudex elongates, and taking a direction diametrically opposite to that of the root, it raises, in a great many kinds of plants, the cotyledons out of the soil, which then become the seminal leaves of the young plant ‡. During this period the young plant derives nutriment almost wholly from the cotyledons or seed-leaves, and if those be destroyed, it perishes. Gravitation by operating on bodies differently organized, and of different modes of growth, appears at once the cause why, in the preceding case, the root descends, and why the elongated plumule ascends §. Lengthening of the caudex.

The bark of the root now begins to execute its office of depositing alburnous or woody matter; and as soon as this is formed, the sap, which had hitherto descended only through the cortical vessels, begins to ascend through the alburnum. The plumule in consequence elongates, its leaves enlarge and unfold, and a set of vessels, which did not exist in the root, are now brought into action. These, which I have called the central vessels, surround the medulla; and, between it and the bark, form a circle, upon which the alburnum is deposited, by the bark, in the form of wedges, or like the stones of an arch ||. Through these vessels, which diverge into the leaf stalks, the sap ascends, and is dispersed through the vessels, and parenchymatous substance of the leaf; and in this organ the fluid, recently absorbed from the soil, becomes converted into the true sap or blood of the plant: and as this fluid, during germination, descended from the Bark of the root. New set of vessels. True sap.

* Ibid. 1809: Journ. vol. XXV, p. 18.

† Phil. Trans. 1809, 1st part, p. 170: Journ. vol. XXV, p. 119.

‡ Phil. Trans. 1806.

§ Phil. Trans. 1st part, 1806, p. 4: Journ. vol. XIV, p. 409.

|| Phil Trans. 1801, plate 27th.

cotyledons

cotyledons and seed-leaves of the plant, it now descends from its proper leaves, and adds, in its descent, to the bulk of the stem, and the growth of the roots. Alburnum is also deposited in the stem of the plant, below the proper leaves, as it was previously deposited below the seed-leaves, and from this spring other central vessels, which give existence to, and feed other leaves and buds *.

Alburnum in the stem with other central vessels.

Fluid absorbed from the soil mixed with sap in the alburnum.

A considerable part of the ascending fluid must necessarily have been recently absorbed from the soil: but in the alburnum it becomes mixed with the true sap of the plant, a portion of which, during its descent down the bark, appears to secrete into the alburnum, through passages correspondent to the anastomosing vessels of the animal economy †. For as the cotyledons, or seed-leaves, first afforded the organizable matter which composed the first proper leaves, so these, when full-grown, prepare the fluid which generates other young leaves, the health and growth of which are as much dependent on the older leaves, as those, when first formed, were upon the cotyledons ‡.

Power of the leaves to generate sap.

The power of each proper leaf to generate sap, in any given species and variety of plant, appears to be in the compound ratio of its width, its thickness, and the exposure of its upper surface to light, in proper temperature. As the growth of the plant proceeds, the number and width of the mature leaves increase rapidly, in proportion to the number of young leaves to be formed; and the creation consequently exceeds the expenditure of true sap. This therefore accumulates during a succession of weeks, or months, or years, according to the natural habits and duration of the plant, and varying considerably according to the soil and climate in which each individual grows: and the sap thus generated is deposited in the bulb of the tulip, in the tuber of the potato, in the fibrous roots of grasses, and in the alburnum of trees, during winter, and is dispersed through their foliage and bark during the spring and summer §.

* Phil. Trans. 1801 and 1805.

† Phil. Trans. 1807, p. 109: Journ. vol. XIX, p. 246.

‡ Ibid. 1805: Journ. vol. XII, p. 233.

§ Ibid. 1809, p. 10: Journ. vol. XXV, p. 123.

As

As soon as the plant has attained its age of puberty, a portion of its sap is expended in the production of blossoms and fruit. These originate from, and are fed by central vessels, apparently similar to those of the succulent annual shoot and leaf stalk, and which probably convey a similar fluid; for a bunch of grapes grew and ripened, when grafted upon a leaf stalk; and a succulent young shoot of the vine, under the same circumstances, acquired a growth of many feet *. Blossoms and fruit.

The fruit, or seed-vessel, appears to be generated wholly by the prepared sap of the plant, and its chief office to be that of adapting the fluids, which ascend into it, to afford proper nutriment to the seeds it contains. I proceed to offer some observations upon the proper culture of the melon †. Fruit.

There is not, I believe, any species of fruit at present cultivated in the gardens of this country, which so rarely acquires the greatest degree of perfection, which it is capable of acquiring in our climate, as the melon. It is generally found so defective both in richness and flavour, that it ill repays the expense and trouble of its culture; and my own gardener, though not defective in skill or attention, had generally so little success, that I had given him orders not to plant melons again. Attending, however, after my orders were given, more closely to his mode of culture, and to that of other gardeners in my neighbourhood, I thought I saw sufficient cause for the want of flavour in the fruit, in the want of efficient foliage; and appealing to experiment, I have had ample reason to think my opinions well founded. Melons not brought to due perfection, from the want of efficient foliage.

The leaves of the melon, as of every other plant, naturally arrange themselves so as to present, with the utmost advantage, their upper surfaces to the light; and if by any means the position of the plant is changed, the leaves, as long as they are young and vigorous, make efforts to regain their proper position. But the extended branches of the melon plant, particularly under glass, are slender and feeble; its Causes of this.

* Phil. Trans. 1803 and 1804: Journ. vol. X, p. 293.

† Ibid. 1801.

leaves are broad and heavy; and its leaf stalks long; so that if the leaves be once removed, either by the weight of water from the watering pot, the hand of the gardener in pruning, in eradicating weeds, or any other cause, from their proper position, they never regain it; and in consequence a large portion of that foliage, which preceded, or was formed at the same period with the blossoms, and which nature intended to generate sap to feed the fruit, becomes diseased and sickly, and consequently out of office, before the fruit acquires maturity.

Attempt to remedy this defect.

To remedy this defect, I placed my plants at greater distances from each other than my gardener had previously done, putting a single plant under each light, the glass of which was six feet long by four wide. The beds were formed of a sufficient depth of rich mould to ensure the vigorous growth of the plant; and the mould was, as usual, covered with brick-tiles, over which the branches were conducted in every direction, so as to present the largest possible width of foliage to the light. Many small hooked pegs, such as the slender branches of the beech, the birch, and hazle, readily afford, had been previously provided; and by these, which passed into the mould of the bed between the tiles, the branches of the plants were secured from being disturbed from their first position. The leaves were also held erect, and at an equal distance from the glass, and enabled, if slightly moved from their proper position, to regain it.

No water thrown on the leaves.

I, however, still found, that the leaves sustained great injury from the weight of the water falling from the watering pot; and I therefore ordered the water to be poured, from a vessel of a proper construction, upon the brick-tiles, between the leaves, without at all touching them; and thus managed, I had the pleasure to see, that the foliage remained erect and healthy. The fruit also grew with very extraordinary rapidity, ripened in an unusually short time, and acquired a degree of perfection, which I had never previously seen.

Superfluous shoots to be pinched off.

As soon as a sufficient quantity of fruit (between twenty and thirty pounds) on each plant is set, I would recommend the farther production of foliage to be prevented, by pinching off the lateral shoots as soon as produced, wherever

more

more foliage cannot be exposed to the light. No part of the full grown leaves should ever be destroyed before the fruit is gathered, unless they injure each other, by being too much crowded together; for each leaf, when full grown, however distant from the fruit, and growing on a distinct branch of the plant, still contributes to its support; and hence it arises, that when a plant has as great a number of growing fruit upon part of its branches, as it is capable of feeding, the blossoms upon other branches, which extend in an opposite direction, prove abortive.

Importance of the leaves.

The variety of melon, which I exclusively cultivate, is little known in this country, and was imported from Salonica by Mr. J. Hawkins. It's form is nearly spherical, when the fruit is most perfect, and without any depressions upon its surface: its colour approaching to that of gold, and its flesh perfectly white. It requires a much greater state of maturity than any other variety of its species, and continues to improve in flavour and richness, till it becomes externally soft, and betrays some symptoms of incipient decay. The consistence of its flesh is then nearly that of a water melon, and it is so sweet, that few will think it improved by the addition of sugar. The weight of a good melon of this variety is about seven pounds. I send some seeds of it to be distributed amongst such members of the Horticultural Society, as may wish to receive them.

The variety cultivated.

VIII.

Some Remarks on Pruning and Draining standard Apple and Pear Trees. By Mr. John Maher, *F. H. S.**

WE often see apple and pear trees, both in gardens and orchards, not only crowded too closely together, but so loaded with their own branches, that very little fruit is produced; and that which is produced is rendered greatly inferior in size and flavour to what it would be under different management.

Apple and pear trees commonly too thick and crowded.

* Trans. of the Hort. Soc. vol. I, p. 236.

Improvement in their management.

Directions for pruning these, as well as all other fruit trees, have already been published by various experienced gardeners, nor is it my present intention to offer any instructions on this head; but necessity, which has been so justly called the mother of invention, having impelled me to try a method that I have not seen practised by any other person, and which has proved uncommonly successful, a short detail of it may perhaps be deemed not unworthy the attention of the Horticultural Society.

Some trees in the above state

When first I came to Millfield, I found a number of apple and pear trees, not only planted too closely, but left entirely to their natural manner of growing, and exceedingly shaded by a row of high trees in the hedge, which separates them from the pleasure ground.

thinned out greatly by pruning.

Other business to be done, of more importance, prevented me from pruning the whole immediately; but a number were selected the first season, and many of their largest branches taken entirely out from the bottom, cutting the wounds very clean. The remaining branches were also properly thinned, so as to leave room for the air and light to play upon the smallest branches.

The shoots from them very vigorous.

The following summer, the shoots pushed from those pruned trees, as might have been expected, were uncommonly vigourous, such as the French call *gourmands*, often from three to five feet long, or more. About the end of June, or a little sooner and later, according to the growth of the branches, I applied oval balls of grafting clay towards their extremity, sufficiently heavy to incline them downwards in a pendulous direction. The sap being thus diverted from its natural mode of ascending and descending, every bud almost became a blossom bud, and in several trees this disposition to produce blossom buds was carried down to the very lowest spurs on the stem and thicker branches.

Bent down by a weight near the end.

Advantages of the practice.

I need not add, that this practice has since been closely followed up; for many advantages, exclusive of a more certain crop of fruit, attend it. 1st. Other small vegetables may be successfully cultivated under the light shade of trees kept so open, an object of importance in the villages near London, where ground is so difficult to be got: 2dly, No expense of

espalier,

espalier, or of stakes, or of training and tying down the branches is incurred: 3dly, The crop of fruit is not only improved in size and flavour by having so much sun and air, but it is more easily gathered, and suffers much less from the autumnal winds; for branches in this direction are more pliable, and bend more easily to the storm; and as a proof how much may be done by art if necessary, the branches of a Lombardy poplar accidentally left in my master's orchard, after being loaded with clay balls, became as pendulous as those of the weeping willow *.

Branches of a poplar rendered pendulous.

I have only to add, that most of the specimens of apples and pears produced at our meeting in November and December last by me, and honoured with the encomiums of some of the best judges present, grew upon trees kept low and open in this method.

The fruit excellent.

IX.

On the Advantages of employing Vegetable Matter as Manure in a fresh State. *By* T. A. KNIGHT, *Esq. F. R. S., Pres. H. S.* †

WRITERS upon agriculture, both in ancient and modern times, have dwelt much upon the advantages of collecting large quantities of vegetable matter to form manures; while scarcely any thing has been written upon the state of decomposition, in which decaying vegetable substances can be employed, most advantageously, to afford food to living plants. Both the farmer and gardener, till lately, thought that such manures ought not to be deposited in the soil till putrefaction had nearly destroyed all organic texture; and this opinion is, perhaps, still entertained by a majority of gardeners: it is, however, wholly unfounded. Carnivorous animals, it is well known, receive most nutriment from the

Vegetable substances best for manure when fresh.

* Our President has shown, in the Philosophical Transactions of 1806, the extensive influence of gravitation upon the motion of the sap of plants; and his experiments perfectly support the author's conclusions.—*Secr.* See Journal, vol. XIV, p. 409.

† Trans of the Hort. Soc. vol. I, p. 248.

flesh

flesh of other animals, when they obtain it most nearly in the state in which it exists as part of a living body; and the experiments, I shall proceed to state, afford evidence of considerable weight, that many vegetable substances are best calculated to reassume an organic living state, when they are least changed and decomposed by putrefaction.

Proof of this in a seedling plum,

I had been engaged in the year 1810, in some experiments, from which I hoped to obtain new varieties of the plum; but one only of the blossoms, upon which I had operated, escaped the excessive severity of the frost in the spring. The seed, which this afforded, having been preserved in mould during the winter, was, in March, placed in a small garden-pot, which was nearly filled with the living leaves and roots of grasses, mixed with a small quantity of earth; and this was sufficiently covered with a layer of mould, which contained the roots only of grasses, to prevent, in a great measure, the growth of the plants which were buried. The pot, which contained about one sixteenth of a square foot of mould and living vegetable matter, was placed under glass, but without artificial heat, and the plant appeared above the soil in the end of April. It was three times, during the summer, removed into a larger pot, and each time supplied with the same matter to feed upon; and in the end of October its roots occupied about the space of one third of a square foot, its height above the surface of the mould being then nine feet seven inches.

and in potatoes.

In the beginning of June a small piece of ground was planted with potatoes of an early variety, and in some rows green fern, and in others nettles, were employed instead of other manure; and, subsequently, as the early potatoes were taken up for use, their tops were buried in rows in the same manner, and potatoes of the preceding year were placed upon them, and covered in the usual way. The days being then long, the ground warm, and the decomposing green leaves and stems affording abundant moisture, the plants acquired their full growth in an unusually short time, and afforded an abundant produce; and the remaining part of the summer proved more than sufficient to mature potatoes of any early variety. The market gardener may, probably, employ the tops

Hint to the

tops of his early potatoes, and other green vegetable substances in this way, with much advantage. gardener.

In the preceding experiments the plum-stone was placed to vegetate in the turf of the alluvial soil of a meadow, and the potatoes grew in ground which, though not rich, was not poor: and, therefore, some objections may be made to the conclusions I am disposed to draw in favour of recent vegetable substances as manures. The following experiment is, I think, decisive. Possible objections to the preceding experiments.

I received, from a neighbouring farmer, a field naturally barren, and so much exhausted by ill management, that the two preceding crops had not returned a quantity of corn equal to that which had been sowed upon it. An adjoining plantation afforded me a large quantity of fern, which I proposed to employ as manure for a crop of turnips. This was cut between the tenth and twentieth of June; but as the small cotyledons of the turnip-seed afford little to feed the young plant; and as the soil, owing to its extreme poverty, could not afford much nutriment; I thought it necessary to place the fern a few days in a heap, to ferment sufficiently to destroy life in it, and to produce an exudation of its juices; and it was then committed in rows to the soil, and the turnip-seed deposited, with a drilling machine over it. A decisive one.

Some adjoining rows were manured with the black vegetable mould obtained from the site of an old wood pile, mixed with the slender branches of trees in every stage of decomposition, the quantity placed in each row appearing to me to exceed, more than four times, the amount of the vegetable mould, which the green fern, if equally decomposed, would have yielded. The crop succeeded in both cases; but the plants upon the green fern grew with greatly more rapidity than the others, and even than those which had been manured with the produce of my fold and stable-yard, and were distinguishable, in the autumn, from the plants in every other part of the field, by the deeper shade of their foliage.

I had made, in preceding years, many similar experiments with small trees (particularly those of the mulberry when bearing fruit in pots) with similar results; but I think it Similar previous experiments.

unnecessary

unnessary to trespass on the time of the Society by stating these experiments, conceiving those I have stated to be sufficient to show, that any given quantity of vegetable matter can generally be employed, in its recent and organized state, with much more advantage than when it has been decomposed, and no inconsiderable part of its component parts has been dissipated and lost, during the progress of the putrefactive fermentation.

X.

Abstract of a Paper on the tanning Substances formed by the Action of Nitric Acid on several vegetable Matters: by Mr. CHEVREUL *.

Mr. Hatchett's artificial tannin.

1. MR. HATCHETT has distinguished three varieties of artificial tanning matter produced, 1st, by the action of nitric acid on any vegetable, animal, or mineral carbonaceous substance: 2d, by its action on common resin, indigo, dragon's blood, &c. †: 3d, by the action of sulphuric acid on camphor, common resin, elemi, &c.

2. In the present paper I shall speak only of the first two varieties of tanning matter, reserving the third for a separate paper.

PART I. *Tanning matter formed with resinous substances.*

§ I. *With Indigo.*

Prepared from Indigo.

Described.

3. This is what I mentioned in my former paper, under the name of "*substance of an oily appearance* ‡." It was of an orange red colour, fluid at a temperature of 15°. [59° F.], but growing thick in the air: it had an acid,

* Ann. de Chim. Vol. LXXIII, p. 36. Read to the Institute, July, 1809.

† Mr. Hatchett having observed, that the most carbonaceous substances were best adapted for conversion into tanning matter, supposed, that, when this matter was formed from resins, these lost a part of their hidrogen, and were thus made to approximate carbonaceous substances.

‡ See Journal, vol. xxx, p. 353.

astringent,

astringent, and bitter taste: it precipitated gelatine copiously, and adhered strongly to animal substances, which it dyed of a saffron colour: it was more soluble in hot water than in cold: it was dissolved by potash, and this compound at the expiration of some days had deposited a small quantity of detonating matter. It was readily soluble in concentrated nitric acid, and in alcohol. I analysed it in the following manner.

4. *a.* I took 40 parts, that I had dried in a capsule with a gentle heat, boiled them in distilled water, and added in three portions 30 parts of carbonate of lead. Effervesence took place. After an hour's boiling, I filtered. Analysis of it.

b. A pulverulent matter, of the colour of bistre, remained on the paper, which, being washed with cold water, acidulated with sulphuric acid, yielded sulphate of lead, a liquor containing some *resin*, and a small quantity of the two *amers.*

c. The filtered liquor (*a*) was decomposed by sulphuric acid. The sulphate of lead being separated, the liquor was concentrated: and a little oily matter was deposited, which had either escaped the action of the carbonate of lead, or was regenerated during the evaporation. The liquor separated from the oily matter, and concentrated farther, left a thick, very bitter, and astringent matter, that precipitated gelatine. This was divided into two portions, No. 1, and No. 2.

d. No. 1 was mixed with potash, which coagulated it almost into a mass. It was then diluted with cold water, and filtered. On the filter was left a deep yellow powder, not crystallizable, and susceptible of detonation, but less so than the compound of amer at a maximum and potash. The aqueous solution of this powder reddened sulphate of iron at a maximum, and precipitated gelatine, when the alkali had been previously saturated with an acid. This yellow powder was a compound of potash, amer at a minimum, and amer at a maximum: for, having decomposed it by muriatic acid, I obtained by spontaneous evaporation, 1st, crystals of amer at a minimum retaining a little amer at a maximum; 2dly, a mother water, which, being saturated with potash, yielded a detonating substance in small yellow needles,

needles, altogether resembling that formed with amer at a maximum.

The liquor from which the yellow detonating powder was separated contained some resin, with the addition of a small quantity of amer at a minimum.

e. No. 2 was treated with concentrated nitric acid. A few small globules of an oily appearance were formed. It was boiled, evaporated to dryness, dissolved in water, and saturated with potash. The compound of amer at a maximum with potash was obtained crystallized. The mother water yielded on evaporation fresh detonating acicular crystals, of a deeper yellow than the first from their retaining a little resin.

Inferences from this.

From these experiments I concluded,

1st, that the substance of an oily appearance is formed of resin, amer at a minimum for the greater part, amer at a maximum, and perhaps nitric acid combined with all three, and contributing to their fluidity: 2dly, that amer at a minimum is capable of combining with amer at a maximum *, and forming a detonating compound with potash.

Rationale of the analysis.

5. The analysis of the matter of an oily appearance is founded on the difference of solubility of amers and resin, and on that of their compounds with the oxide of lead. Thus, when carbonate of lead is boiled with the oily matter, the amers and a small quantity of resin combine with the metallic oxide, and form compounds soluble in water: and as soon as the greater part of the resin is freed from the amers, it is much less soluble, and consequently will separate; and its separation is farther promoted by its combining with a little oxide of lead. The resin, from its affinity for the amers, carries down a small quantity with it. When the soluble compound of amers with lead is decomposed by sulphuric acid, the two amers combine together, so that they jointly form a compound of little solubility with potash; and when this compound is treated with muriatic acid, the

* The compound of the two amers has appeared to me capable of assuming the form of globules of an oily appearance at a temperament of + 60° [140° F.] Perhaps the nitric acid contributes to give it this state.

the amers separate from each other in the order of their respective solubilities.

6. I do not consider the substance of an oily appearance as uniform in the proportions of its immediate principles. Sometimes the amer at a minimum is in small quantity: sometimes it predominates over the others. The same may be said of the amer at a maximum, and of the resin. This is the reason of the differences observed in the colour, consistence, and other properties of these compounds.

The proportions of the immediate principles not constant.

7. It remains for me to assign the reason why the compound of resin and amers precipitates gelatine much more copiously than amer at a maximum. The fact appears to me owing to this, that the resin and amer at a minimum (which have themselves an affinity for animal substances, but not sufficient to precipitate gelatine) in combining with amer at a maximum diminish the solubility of the latter, solidify it in some degree, and thus in all probability increase the capacity it has of forming with gelatine a compound but little soluble.

Why the compound precipitates gelatine.

§ II. *Bitter tanning substance formed with extract of brasil wood.*

8. In my first paper on brasil wood, I described a bitter substance, produced by the action of nitric acid on the extract of this wood. I showed, that this substance did not crystallize, was acid, precipitated gelatine, and formed with potash small detonating crystals. At that time I considered this substance as a compound of amer, artificial tannin, and nitric acid. My reasons were, 1st, its being fusible by heat, and forming detonating salts with salifiable bases, in the manner of Welther's amer: 2dly, its precipitating gelatine in the manner of the tannin described by Mr. Hatchett, while the amer of Welther, prepared with indigo, does not precipitate it; whence I inferred, that two different substances were requisite, to form detonating compounds and precipitate gelatine*: 3dly, its reddening litmus paper; because, in treating extract of brasil with nitric acid, I had obtained

Tannin from extract of brasil.

Supposition respecting its nature.

* The reason of my not obtaining a precipitate with Welther's amer and gelatine at that time was my employing solutions too dilute, or adding too much gelatine.

another

another substance, which did not perceptibly redden litmus, which was fusible by heat, and which precipitated gelatine. I concluded the account of my labours with saying, that I purposed to examine whether the tanning matter of Hatchett were a real tannin, whether the amer of indigo (or of Welther) were similar to that of brasil, and lastly, whether these substances owed their detonating property to nitric acid.

Amer of brasil prepared with potash.

9. To solve these questions I prepared a certain quantity of amer of brasil in the following manner. I combined it with potash; I boiled this compound in water acidulated with muriatic acid; by evaporation I obtained a whitish substance crystallized confusedly; and the mother water, from which this was separated, yielded by concentration fresh crystals, mixed with small grains of a resinous appearance, and of a reddish colour. This experiment led me to think, that, if the amer of brasil be incapable of crystallizing before it is combined with potash, it is because it is combined with a certain quantity of matter, which I believe to be resinous. The latter contributes also to give it the property of precipitating gelatine more abundantly than the amer of indigo.

Approaches that of indigo, but not the same.

The crystallized amer of brazil forms with potash a detonating salt of a much lighter colour, than that which it forms when not crystallized. It seems then to approach the amer of indigo; but it exhibits certain differences, which do not permit them to be confounded together.

Its destructive analysis.

10. The crystallized amer of brasil, heated in the glass tube with a bulb before mentioned *, yielded, 1st, water; 2d, carbonic acid; 3d, prussic acid; 4th, an inflammable gas, that burned with a heavy white flame, in the manner of oily hidrogen; 5th, nitrous gas; 6th, nitrogen gas; 7, a coal, extremely attenuated.

A compound of nitric acid and an inflammable substance.

Hence I concluded, that the amer of brasil is a compound of nitric acid, and a substance apparently resinous or oily: that this compound, when united with salifiable bases, forms detonating salts: that it precipitates gelatine more copiously than the amer of indigo does, because it appears

* Journal, vol. xxx, p. 354.

to

to have a greater tendency to solidity than the latter; and that the presence of a certain quantity of resin, with which it may be united, favours this precipitation.

§ III. *Tanning substance formed with aloes.*

11. On treating aloes after the mode of Mr. Braconnot, I obtained the matter he called aloetic acid *. Tannin from aloes.

This acid substance has a yellow colour; an acid, astringent, and bitter taste: projected on a hot iron, it emits a yellow smoke, is carbonized, and melts. Heated in the glass bulb it yields, 1st, water; 2d, carbonic acid; 3d, prussic acid; 4th, an inflammable gas, which I believe to be a mixture of oily hidrogen and gaseous oxide of carbon; 5th, nitrogen gas; 6th, charcoal.

Hence it appears to me, that the aloetic acid is a compound of nitric acid analogous to those I have already described. A similar compound.

This substance is but little soluble in water; and, though it is yellow, its solution is of a fine purple. I imagine the water acts in this case by weakening the action of the nitric acid on the vegetable matter with which it is combined. This purple solution becomes yellow on the addition of nitric acid, muriatic acid, &c. Its properties

It gives a purple colour to alcohol.

I boiled some in nitric acid at 50° [sp. gr. 1·5], and evaporated to dryness. On dissolving the residue in water, I obtained a red solution, inclining a little to fiery, because probably there remained a little excess of acid. It seems to me, therefore, that the nitric acid had not changed the nature of this substance. not altered by nitric acid.

With salifiable bases it forms purple compounds, which are detonating, as Mr. Braconnot first observed. Its compounds with the bases.

I have found, that it precipitates gelatine very well; and even that several of its soluble compounds precipitate it without the addition of an acid, which Welther's amer combined with potash never does, when it contains no resin. Precipitates gelatine.

12. Is the aloetic acid a compound of nitric acid and a substance arising from the decomposition of aloes? or a compound of nitric acid and the coloured principle of aloes Its nature.

* Journal. vol. xxvii, p. 365.

little

little or not at all changed? The colour of this substance leads me to incline to the latter opinion: yet I do not consider it as impossible, that, beside the acid and coloured principle, it may contain a portion of aloes with its nature changed; for the pretty copious production of oxalic acid in the treatment of aloes by nitric acid proves, that a part of the aloes is completely decomposed.

Hypotheses.

13. Perhaps it may be thought, that the amers of brasil and aloes are only compounds of amer at a maximum, nitric acid, and substances resulting from the more or less advanced decomposition of the articles with which they were formed. Without venturing to assert, that this opinion is absolutely false, it appears to me more natural at present, to consider these amers as two distinct species of amer at a maximum. Hence it follows, that resinous substances treated with nitric acid do not afford a homogeneous principle, that may be considered as a kind of artificial tannin. Besides, the following experiments will prove, that the faculty of precipitating gelatine belongs to substances of a very different nature, and in which the presence of amer at a maximum cannot be suspected.

Different substances precipitate gelatine.

Tannin from pitcoal.

Part II. *Tanning matter formed with carbonaceous substances.*

§ I. *With pitcoal.*

Mr. Hatchett's experiments on bitumens

1. Mr. Hatchett asserts, that several bitumens, as jet and asphaltum, are formed of charcoal and a resinous matter; and that, when nitric acid is digested on these compounds, the carbonaceous part dissolves, and the resinous part separates in the form of a yellow or orange-coloured mass. Mr. Hatchett applies this discovery to pitcoal; and says, that when this contains no resinous substance, which is the most common case according to him, it is completely dissolved by nitric acid, and converted into tannin; and that, on the contrary, when it contains a little resinous matter, this is not dissolved. The results I have obtained differ a little from those of Mr. Hatchett. Like him, by treating pitcoal with concentrated nitric acid, and reducing the liquor to a sirupy consistence, I obtained a thick, brown homogeneous liquid; but when this liquid was poured into water, a yel-

and coal.

The author's results somewhat different.

a yellow matter separated, which was much more abundant than what remained in solution, and had no property, that rendered it similar to resins. I obtained the same results with two varieties of pitcoal, yet I do not allow myself the least reflection on the labours of that celebrated English chemist; as I am too fully aware, that different modes of operating, and the different varieties of the bodies examined, are so many causes, that may produce a variation in the results. I shall proceed therefore to relate my own experiments, and deduce from them the conclusions, that appear to me most natural.

Causes of difference in results.

2. The pitcoal I used was perfectly pure. 100 parts heated strongly in a platina crucible left 84 parts of coak.

Pitcoal used.

I digested 100 parts of this coal, finely powdered, in 600 parts of nitric acid at 44° [sp. gr. 1·425]. An effervescence took place, with the evolution of nitrous vapour, &c. When the action diminished I increased the heat; and at the expiration of 24 hours I added 600 parts of nitric acid, and heated to boiling, taking care to pour back into the retort the acid that passed over into the receiver. Finally, when the matter appeared to be thoroughly attacked, I poured it out into a capsule, and evaporated gently to dryness. The residuum weighed 170 parts, consequently there was an increase of weight of seven tenths. The hot water, with which I washed it repeatedly, acquired a reddish brown colour, and an acid astringent taste, from dissolving the tannin of Mr. Hatchett. The yellow and little soluble matter, which I shall designate by the letter A, was not dissolved.

Treated with nitric acid.

Art. I. *Examination of the tanning matter of Mr. Hatchett.*

3. I evaporated to dryness the washings of the pitcoal treated by nitric acid (2), redissolved the residuum in a small quantity of water, and thus separated a little of the matter A. The filtered liquor had an acid taste, with a little bitterness and astringency: it coagulated gelatine very well. To separate the tanning substance in a state of purity, I poured into it acetate of lead, till no more precipitate was thrown

Examination of the artificial tannin.

thrown down. I then poured off the liquid, which was of a light yellow, and washed the precipitate with a great deal of water.

4. This precipitate, which was a compound of the tanning matter and oxide of lead, was thrown still wet into water acidulated with sulphuric acid. After boiling I left these substances to act on each other for twenty-four hours. At the expiration of this time I satisfied myself by means of barytes water and sulphuretted hidrogen, that there was neither sulphuric acid nor lead in the solution.

Its properties.

I filtered the liquor to separate the sulphate of lead, and evaporated it to dryness. A brown mass remained, which melted by heat, hardened on cooling, and afterward attracted moisture from the atmosphere. The aqueous solution of this substance reddened litmus, and formed a precipitate with gelatine, barytes water, and acetate of lead. The precipitates with the latter two were soluble in nitric acid; and melted when exposed to heat in a glass tube closed at one extremity, emitting an aromatic smell mixed with something of the prussic. When operating on the precipitate with lead, if the residuum were thrown on a paper while hot, it took fire like a pyrophorus. This combustion was produced by charcoal and metallic lead in a state of minute division. The residuum of the compound with barytes was very little pyrophoric.

Examination of it for nitric acid.

5. To ascertain whether any nitric acid were present in the tanning matter prepared by the preceding process, I introduced 5 dec. [7·7 grs] into the glass bulb, and heated them. The matter fused, because it contained a little humidity; and evolved with much impetuosity aqueous vapour, ammonia, carbonic acid, nitrous gas, &c. A coal remained, that emitted a strong smell of prussic acid.

Combination of it with sulphuric acid.

6. As I tried the preceding experiment several times, I found, that sulphuric acid was capable of combining with the tanning matter, when it separated it from oxide of lead; and that this compound, when it did not contain an excess of sulphuric acid, formed with barytes a precipitate soluble in nitric acid, and gave out sulphuric acid when heated. It seemed to me, that by boiling carbonate of lead with this compound dissolved in water, evaporating to dryness and redissolving

redissolving in water repeatedly, the oxide of lead united with the sulphuric acid, and a substance was obtained, which when heated no longer gave out any sensible quantity of sulphurous acid. This experiment I performed but once.

Amer found in the liquid from which the tannin had been separated.

7. The liquor, from which the tanning matter had been separated by acetate of lead, had sulphuretted hidrogen passed through it; after which it was filtered, and evaporated to dryness. The residuum was dissolved in water, and potash was added to the solution. This produced a yellow precipitate of lime retaining some bitter matter. The liquor being filtered and concentrated yielded silky crystals, of a golden yellow colour, detonating, and resembling those formed by Welther's amer and potash. Proust had already observed, that a small quantity of this substance was formed, when pitcoal was treated with nitric acid at 40° [sp. gr. 1·396].

Nature of the compound.

Hence it follows, that the matter soluble in water is formed, 1st, of a substance that precipitates gelatine copiously, which is a compound of nitric acid, and carbonaceous matter; 2d, of a very small quantity of amer at a maximum. The acetate of lead forms with the first a compound insoluble in water, and with the second a compound soluble in it.

Art. II. *Examination of the matter* A.

Examination of the difficultly soluble matter.

8. The matter A, after it had been several times washed, was of the colour of umber. It had a slightly acid taste; and reddened litmus paper on which it was moistened with a little water. Heated in a glass tube it melted, emitting a red light, and a smell of nitrous acid mixed with prussic. To destroy the supposition, that the nitrous acid might have arisen from the remains of the acid that escaped the waters of elutriation, I digested the matter A in water, filtered, and washed it repeatedly with fresh water. Of the substance thus washed I heated 2 dec. [3 grs] in the glass bulb. The matter fused, and gave out, 1st, water; 2d, nitrous vapour; 3d, carbonic acid; 4th, ammonia; 5th, some inflammable gas, which appeared to me a mixture of oily hidrogen and gaseous oxide of carbon, for it burned with a heavy white flame, and presently with a blue; 6th, nitrous gas;

gas; 7th, nitrogen gas*; 8th, prussic acid, sensible to the smell, but in too small quantity to afford prussian blue; 9th, charcoal.

9. The matter A digested with a small quantity of water coloured it red, and gave it the property of precipitating gelatine. The residuum boiled with fresh water was in part dissolved; and ultimately left a blackish substance, heavier than the matter A, and very slightly colouring water with which it was boiled. I believe it was nothing but the oxide of carbon described by Proust. To this I shall presently return.

10. The washings of the matter A were concentrated by a gentle heat. A substance was deposited, apparently very similar to A, and a very astringent matter remained in the concentrated liquor.

Separated into three substances.

Hence it follows, that water separated the matter A into three different substances: 1st, a black substance, nearly insoluble in water, which I shall call A^1: 2d, a substance soluble in water, but precipitable from it by evaporation, which I shall denote by A^2: 3d, a substance very soluble in water, A^3.

The insoluble substance examined.

11. A^1 was a little acid. 5 dec. [7·7 grs], heated in the glass bulb, melted, diffusing a red light, and giving out, 1st, water; 2d, carbonic acid; 3d, inflammable gas, burning white; 4th, nitrous gas; 5th, nitrogen gas; 6th, a little ammonia; 7th, a coal, that emitted a strong smell of prussic acid.

A compound of nitric acid and carbon.

Hence it is evident, that this substance, which possesses the properties ascribed by Proust to the oxide of carbon, is a compound of nitric acid and carbon: it differs from A^2 and A^3 only by containing less acid: and what appears to confirm this is, by boiling it in concentrated nitric acid it is totally dissolved; and, when water is poured into this solution, it throws down a yellow flocculent precipitate, exhibiting all the properties of the unwashed matter A. Hence I imagine, that, when the matter A is boiled in water, the portion

* With respect to this product see what I have said in the article of the decomposition of Welther's amer by heat in my paper on the amers from indigo. See Journ. vol. XXX, p. 351.

portion which does not dissolve cedes a part of its acid to that which dissolves; and, when the washings are afterward evaporated, a farther division is made of the acid between the substance A^2, which is precipitated, and A^3, which remains in solution.

Attempt to convert it into tannin.

Nitric acid, at least in the proportion in which I employed it, could not convert A^1 into the tanning matter of Hatchett, which is very soluble in water. There is a portion of matter, however not separated by water from the nitric solution of A^1, which precipitates gelatine; but I cannot assert, that it is absolutely similar to the matter of Hatchett.

The nitric acid not separable from it by carbonate of potash.

To find whether it were possible, to remove the nitric acid from A^1 without heating it, I digested it in a weak solution of neutral carbonate of potash. By the assistance of heat carbonic acid was evolved, and nearly the whole was dissolved.

This solution was decomposed by sulphuric acid, which threw down a brown flocculent precipitate. The supernatant liquid was colourless. It was filtered: the slight excess of sulphuric acid contained in it was saturated with carbonate of potash: it was evaporated to dryness, and the residuum was treated with alcohol at 30° [sp. gr. 0·868], to dissolve the nitre, if it contained any; but none was found. The carbonate of potash therefore had taken no observable quantity of nitric acid from A^1.

After it is separated from potash by sulphuric acid rather more soluble,

The brown precipitate left on the filter was washed with hot water, till this gave no farther indication of sulphuric acid to the test of solution of barytes. At this period the water of elutriation was fawn-coloured, had a taste and smell slightly inclining to those of oak bark and roses, and did not perceptibly precipitate gelatine. On adding an acid, a little flocculent precipitate fell down.

from being more minutely divided.

If water dissolve more of A^1 that has been precipitated from potash by sulphuric acid, than of that which has not, I believe it depends on its being more minutely divided: for in that which I prepared with care I found no sensible quantity of sulphuric acid *, and its coal afforded only an atom of potash.

* Experiments I have since made lead me to think, that potash contributes to the solution of this substance in water.

5 dec. [7·7 grs] of A^1, which had been dissolved by the carbonate, and afterward precipitated by sulphuric acid, melted with heat, and afterward gave out carbonic acid gas, nitrous gas, &c., leaving a coal, that emitted a smell of prussic acid, and contained an atom of potash.

Examination of the matter precipitated by concentration.

12. A^2, which dissolved in the water of elutriation of A, and afterward fell down during its evaporation (10), was of a blackish brown colour. Treated with boiling water part was dissolved, and imparted to the water the property of coagulating gelatine. The solution yielded by evaporation a residuum, that melted, and evolved nitrous gas. The part but little soluble in water greatly resembled A^1. It melted, and gave out nitrous gas, but in smaller quantity than the portion that had dissolved in the water. This indicates, that acid was transferred from the portion but little soluble to the other.

Examination of the very soluble substance.

13. A^3, which remained in solution after the concentration of the washings of A, and had been obtained by evaporating them, was fawn-coloured. Heated in the glass bulb it melted; yielded water, carbonic acid, nitrous gas, &c.; and left a coal, that emitted a very strong smell of carbonate of ammonia.

Its difference from artificial tannin.

This substance, which precipitated gelatine very well, differed from Hatchett's tannin (Art. I) in its alkaline solution being precipitable by acids, in its being consequently less soluble in water, and in its not melting by heat.

The three differ only in their proportions of nitric acid.

39. The matter A therefore is divisible by water into three portions, which differ from each other only by the quantity of nitric acid they contain, since by taking a portion of this acid from those that contain the most they are converted into those that contain the least; and by adding acid to those that have lost it, they are brought back to their former state.

§ II. *Tanning matter formed with fir charcoal.*

Artificial tannin from charcoal.

40. A hundred parts of fir charcoal, which had been calcined in a platina crucible, in a red heat, required for their solution in nitric acid more time and more acid than 100 parts of pitcoal. The solution of the charcoal was brown, and thick like a sirup. When water was added, a brown matter

matter separated, which I shall examine below. The liquor freed from this was evaporated to dryness. The residuum was black, a little astringent, and slightly acid. Heated in a glass tube, it did not melt, but an acid vapour was evolved. The greater part dissolved in distilled water. This solution precipitated gelatine, and many metallic salts. The precipitate formed with acetate of lead, being heated in a glass tube, left a coal mixed with metallic lead, which took fire, if thrown on paper while hot.

41. To obtain the tanning matter in a state of purity, I precipitated the solution by acetate of lead, and washed the precipitate, till the water that came off ceased to be coloured by sulphuretted hidrogen. I decomposed the precipitate, while yet wet, by sulphuric acid. The sulphate of lead was separated by the filter. With barytes and acetate of lead the liquor threw down a flocculent precipitate, soluble in an excess of nitric acid; which indicated, that it contained no sensible excess of sulphuric acid *. However, having evaporated to dryness, I obtained a brown, deliquescent residuum †, fusible by heat; which, being heated in the glass bulb, gave out carbonic acid gas, sulphurous acid gas, and other gasses insoluble in water, which I was unable to examine from the smallness of their quantity (for I operated only with 2 dec. [3 grs] of tanning matter); so that I know not whether any nitrous gas were among them.

Purified, and examined.

42. Thus it appears, that, when the compound of tanning matter and lead is decomposed by sulphuric acid, the latter,

Sulphuric acid combines with the tanning matter.

* To find whether a liquid contain any excess of sulphuric acid, solution of barytes should be poured into it. If a precipitate form, try to dissolve it in pure nitric acid. If this dissolve it, the matters should be left to act on each other for 24 hours, and then see whether there be any precipitate. I have often observed that lead indicated no sulphuric acid, where barytes did perceptibly. The latter therefore is preferable as a test to the former.

Barytes recommended as a test of sulphuric acid.

† I have found, that by heating this residuum a little strongly in contact with the air in a capsule white fumes of sulphuric acid were evolved; and the substance thus heated, being redissolved in water, gave indications of sulphuric acid, when tested with solution of barytes.

latter, if in excess, enters into combination. It is probable, that the sulphuric acid combines with the tanning matter, without expelling the nitric acid.

Earthy matter of the coal, and yellow bitter matter.

43. The liquor from which the tanning matter had been precipitated by acetate of lead having had sulphuretted hidrogen passed through it, and been afterward filtered, contained the earthy matters of the coal, and a yellow bitter matter, the nature of which I could not positively ascertain.

Brown matter insoluble in water.

44. I dissolved the brown matter I have mentioned (40) in nitric acid at 45° [sp. gr. 1·435], concentrated the solution, and afterward added water to it. This precipitated a yellowish substance, similar in appearance to the matter A, separated by water from a nitric solution of pitcoal: but the yellowish substance differs from A in being entirely soluble in boiling water, and in not being fusible by heat. I presume, that it differs from the portion soluble in water (41) only in containing less nitric acid, and perhaps more hidrogen.

XI.

Chemical Examination of the Husks of Walnuts. By Mr. HENRY BRACONNOT, *Prof. of Nat. Hist. &c.**

Husks of walnuts used in dyeing.

THE daily use of the husks of walnuts in the art of dyeing suggested to me the wish of making some experiments on them, to be enabled to form a more accurate judgment of their nature.

Soon changed, unless kept from air. Apparently a slow combustion effected.

When fresh the husk is interiorly white, but it becomes coloured very quickly, and ultimately passes to a dark brown. This is owing to the contact of the air; for, if it be immersed in water that has been boiled, it will keep some time without undergoing the least change. If it be placed in a jar filled with atmospheric air, the oxigen will soon be converted almost wholly into carbonic acid; the husk acquires a blackish colour; and no doubt there is also a production of water: so that the whole seems to indicate the

* Ann. de Chim. vol. LXXIV, p. 303.

the phenomena of a slow combustion. Oximuriatic acid appears to have another kind of action on it; for, instead of blackening it, it causes it to assume a yellow colour. Nitric acid comports itself in the same manner.

Action of oximuriatic and nitric acid.

To proceed to the examination of the matters contained in the husk, I bruised a certain quantity in a marble mortar, expressed the juice, and filtered it. Some green feculæ remained on the paper, which soon changed to a deep brown by exposure to the air. This matter, washed and dried, was macerated in alcohol, which extracted from it the green resin common to most vegetables. The residuum insoluble in alcohol was still coloured, and felt smooth. A portion of it was diluted with weak nitric acid, which converted it into a thick substance, viscous, and soluble in water. In this solution alcohol occasioned a white flocculent precipitate. The same coloured residuum, being diluted with water to which a little potash was added, produced a bulky tremulous substance, of a deep red colour, and resembling the coagulum of blood. Lastly another portion of the same residuum was dissolved in boiling water, and formed starch. Hence it follows, that this substance, contained pretty abundantly in the husk, is starch contaminated by the colouring matter.

Expressed juice filtered.

Residuum examined.

The juice of the husk recently filtered is of an amber colour, and of an acrid and sour taste mixed with bitterness. This acrid principle appears extremely destructible, for the recent juice, left to itself some days, while it loses its yellow colour to assume a blackish brown where it has been in contact with the air, loses also its acrimony, and becomes decidedly acid: at the same time black pellicles form on its surface, which are soon replaced by others if removed. These pellicles, carefully collected and well washed, yielded on drying a black, brittle substance, of a shining, vitreous fracture, and pretty similar to asphaltum, or Jew's pitch, but burning without any apparent flame, in which it more resembled charcoal. This carbonaceous matter was dissolved in potash, and in this solution a flocculent precipitate was produced by acids. It may be obtained more readily by evaporating the juice of the husk with a gentle heat, and diluting the residuum with water. The

Examination of the filtered juice.

liquid

liquid standing on the sediment is a pure and even agreeable acid; whence it follows, that the acrid and bitter principle has been entirely destroyed, being converted apparently into the black matter approaching the state of charcoal. The same extract gave out no acetous vapour with sulphuric acid, even heated: it contains therefore no acetic acid.

A compound of carbon and hidrogen dissolved in the juice, as in that of many plants.

From what has been said we cannot but observe in the husk of the walnut, as in many herbaceous plants, a substance held in solution in its juices; and the hydrocarburet radical of which is more or less decomposable by the simple contact of air, which appears to cause a production of water, rendering the carbon predominant. It is obviously impossible to have a very accurate idea of a substance so little permanent: but it appears, that it is but slightly coloured in the vessels of plants; and that the action of the air or of caloric alters it greatly; causing it to pass by degrees to the state of extract, another principle badly defined, of little stability in respect to its element, and which seems rather the result of a decomposition, than a real product of living nature*.

Effects of tests on the juice.

The juice of the husk examined by reagents exhibited the following effects.

Litmus.

It strongly reddened infusion of litmus.

Gelatine.

Solution of gelatine formed in it a slight precipitate, which must have been owing to tannin.

Extracts altered by keeping.

* Having had an opportunity of examining some extract of rhus toxicodendron, that had been prepared several years before, I made the following observation. I applied some to the skin of an animal, and gave him some internally in pretty large doses, without his experiencing any troublesome consequences; while one drop from the stalk of the plant on the skin occasioned a tolerably extensive inflammation, terminating in an ulcer. Thus it appears, that the principles of plants condensed to the state of extract undergo an alteration, which continues progressive with time; and this must cause their action on the animal economy to vary greatly.

They should be kept dry, and air excluded.

Perhaps apothecaries may prevent this alteration in a certain degree, by enclosing their extracts, when perfectly dry, in vessels well stopped; for the moisture they contain, or have a tendency to absorb, does not contribute less to alter the feeble equilibrium of some of their elements, than the contact of air.

Sulphate

Sulphate of iron gave the juice so deep a green, that it appeared black. No precipitation took place, even on standing some time, in consequence of the free acid found in the mixture, which is capable of imparting a fine gray to wool or silk. Sulphate of iron.

Oxalate of ammonia indicated the presence of lime. Oxal. of ammonia.

Nitrate of barytes produced no signs of any sulphate. Nitr. of barytes,

Nitrate of silver acts on it in a manner well adapted to reveal the presence of the alterable hydroearburet radical, for it produces a pretty copious precipitate, which quickly becomes coloured; while the silver resumes its metallic lustre from the action of the vegetable substance on the oxigen of the oxide. The precipitate is then no longer soluble but in part in nitric acid, and leaves charcoal as a residuum. and of silver.

Alkalis change the juice to a deep red, and form in it precipitates that contain lime. If after a certain time an acid be poured into the liquor, another flocculent sediment is produced, which dries, grows black, has a vitreous fracture, and resembles in its nature the pellicles, that are formed successively on the surface of the juice exposed to the air. Alkalis.

Acetate of lead occasioned in the juice a whitish, flocculent, very copious precipitate, which dissolved entirely in distilled vinegar. This precipitate, being decomposed by sulphuretted hidrogen, yielded a coloured liquor, of considerable sourness mixed with astringency, which produced a sediment with gelatine, and with acetate of lead a precipitate soluble in vinegar. This acid, being evaporated by a gentle heat, yielded small, ill-defined crystals, immersed in the uncrystallizable liquor. The whole was mixed with carbonate of lime; and after the mixture, which contained an excess of acid, had been heated, I filtered it. By evaporating I obtained a granular, coloured substance, formed by the union of a number of small acicular crystals. This salt, being treated with cold water, dissolved in it in part: and the solution, evaporated to dryness, left a brown varnished residuum, which comported itself like malate of lime, retaining some tannin, which then precipitated iron of a blackish blue. The portion of the calcareous salt that would Acetate of lead.

would not dissolve in cold water was treated with diluted sulpuric acid, which separated from it citric acid, still contaminated with malic.

Subacetate of lead.

The juice thus freed from part of the matters it held in solution was still coloured. Acetate of lead supersaturated with oxide produced in it another sediment, and rendered the supernatant liquor nearly colourless. This sediment yielded on analysis the same products as above; namely malic acid, colouring matter, and tannin, which had escaped the first precipitation in consequence of the presence of the acetic acid, that had become predominant in the liquor.

Examination of the magma.

The magma left after expression of the juice, after having been treated with alcohol, which extracted from it some green resinous matter, was heated with water till it boiled, to free it from the starch and the coloured matter it retained. When thus exhausted, it was digested with dilute nitric acid, which separated some phosphate and oxalate of lime, that had been precipitated from the acid liquor by ammonia. The means I employed to separate these two earthy salts, which are very frequently associated together in vegetables, are founded on the property distilled vinegar diluted with water has of dissolving phosphate of lime, without sensibly affecting the calcareous oxalate.

Distilled water from it.

Though the husk has a peculiar smell, it afforded nothing very remarkable by distillation in a water bath. I obtained only a liquor with a faintish taste, which, instead of coming over limpid, was brownish; and on its surface were perceptible slight iridescent pellicles, which sunk to the bottom in the form of a sediment.

Ashes.

The husk yielded by incineration potash, carbonate of lime, phosphate of lime, and oxide of iron.

From this examination it appears, that the fleshy covering of the walnut contains:

Substances contained in the husk.

1st, Starch:

2d, An acid and bitter substance, very alterable, which appears to approach the state of charcoal by the contact of air:

3d, malic acid:

4th, tannin:

5th, citric

5th, citric acid:
6th, phosphate of lime:
7th, oxalate of lime:
8th, potash.

XII.

Analyses of Minerals: by MARTIN HENRY KLAPROTH, *Ph. D. &c.*

(*Continued from p.* 312.

PHOSPHORESCENT earth from Marmarosch.

Phosphorescent earth of Marmarosch.

Phosphoric acid	32·25
Fluoric acid	2·50
Lime	47
Silex	0·50
Oxide of iron	0·75
Water	1
Quartz mixed	11·50
	95·5

A new combustible mineral from East Prussia. 1000 grs. yielded by distillation.

Combustible mineral from Prussia.

	cubic inches.		Grs.
Carbonic acid gas	130	=	61·1
Carburetted hidrogen gas	320	=	59·5
Empyreumatic oil			90
Carbonate of ammonia			26·5
Water			385·5
The residuum consisting of			
Charcoal			228
Silex			45·5
Oxide of iron			14·5
Alumine			6
Phosphate of lime			14
Sulphate of lime			5
			935·6

A mi-

Mineral water of Riepoldsa.

A mineral water at Riepoldsa in Furstemberg, 128 oz. yielded

	grs.		grs.
Sulphate of soda, dry	93	or crystallized	221·5
Muriate of soda, dry	5		
Carbonate of soda, dry	2		5·5
——— lime	81		
——— magnesia	2		
Oxide of iron	2		
Silex	3		
Carbonic acid gas 332 cub. in.			
	188		

Tantalite.

Tantalite (tantale oxidé ferro-manganésifère of Haüy).

Earth of tantalium	88
Oxidulated iron	10
Oxide of manganese	2
	100

Cyanite.

Cyanite from Airolo, on St. Gothard.

Alumine	55·5
Silex	43
Oxided iron	0·5
Potash, *a trace*.	
	99

Vitreous feld-spar.

Vitreous feldspar, called sanidin, from Drachenfels;

Silex	68
Alumine	15
Oxide of iron	0·5
Potash	14·5
Loss	2
	100

Agalmatolite.

Agalmatolite from Nagyag.

Silex	54·50
Alumine	34
Oxide of iron	0·75
Potash	6·25
Water	4
	99·5

Soaprock

Soaprock from Cornwall. Soapstone.

Silex	45
Magnesia	24·75
Alumine	9·25
Oxide of iron	1
Potash	0·75
Water	18
	98·75

Axinite. Axinite.

Silex	50·50
Lime	17
Alumine	16
Oxide of iron	9·50
——— manganese	5·25
Potash	0·25
	98·5

Gray semiopal from Moravia. Gray semiopal.

Silex	85
Alumine	3
Oxide of iron	1·75
Charcoal	1
Water, a little ammoniacal	8
Bituminous oil	0·33
	99·08

Bronzite *. Bronzite.

Silex	60
Magnesia	27·5
Oxide of iron	10·5
Water	0·5
	98·5

* See Journal, vol. xxv, p. 381.

Hyperstene

Labrador hornblende.

Hypersten (Labrador hornblende of Werner)*.

Silex	54·25
Magnesia	14
Alumine	2·25
Lime	1·50
Oxide of iron	24·50
Water	1
Oxide of manganese, *a trace*	
	97·5

Zoisite.

Zoisite.

Silex	44
Alumine	32
Lime	20
Oxide of iron	2·5
——— manganese, *a trace*	
	98·5

Natrolite.

Natrolite.

Silex	48
Alumine	24·25
Oxide of iron	1·75
Soda	16·50
Water	9
	99·5

Stangenstein.

Pycnite †.

Silex	43
Alumine	49·5
Oxide of iron	1
Fluoric acid	4
Water	1
Loss	1·5
	100

* See Journ. vol. xxvii, p. 153. † Ib. p. 154.

Lamellar

Lamellar talc from St. Gothard *. Lamellar talc.

Silex	62
Magnesia	30·50
Oxide of iron	2·50
Potash	2·75
Loss in roasting	0·50
	98·25

Common mica from Zinnwalde †. Common mica.

Silex	47
Alumine	20
Oxide of iron	15·50
——— manganese	1·75
Potash	14·50
	98·75

Muscovy glass, or mica in large laminæ from Siberia ‡. Mica in large laminæ.

Silex	48
Alumine	34·25
Oxide of iron	4·50
Magnesia	0·50
Potash	8·75
Loss in roasting	1·25
	97·25

Black mica from Siberia §. Black mica.

Silex	42·5
Alumine	11·5
Magnesia	9
Oxide of iron	22
——— manganese	2
Potash	10
Loss in roasting	1
	97

* See Journ. vol. xxvii, p. 226. † Ib. p. 227.
‡ Ib. p. 228. § Ib. p. 230.

Black

Black staurolite.

Black staurolite *.

Silex	37·50
Alumine	41
Oxide of iron	18·25
——— manganese	0·50
Magnesia	0·50
	97·75

Red staurolite.

Red staurolite from St. Gothard †.

Silex	27
Alumine	52·25
Oxide of iron	18·25
——— manganese	0·25
	97·75

Reddish tourmalin from Moravia.

Rubellite from Roschna, where it is found with lepidolite ‡.

Silex	43·50
Alumine	42·25
Oxide of manganese	1·50
Lime	0·10
Soda	9
Water	1·25
Loss	2·40
	100

Blue calcareous stone from Vesuvius.

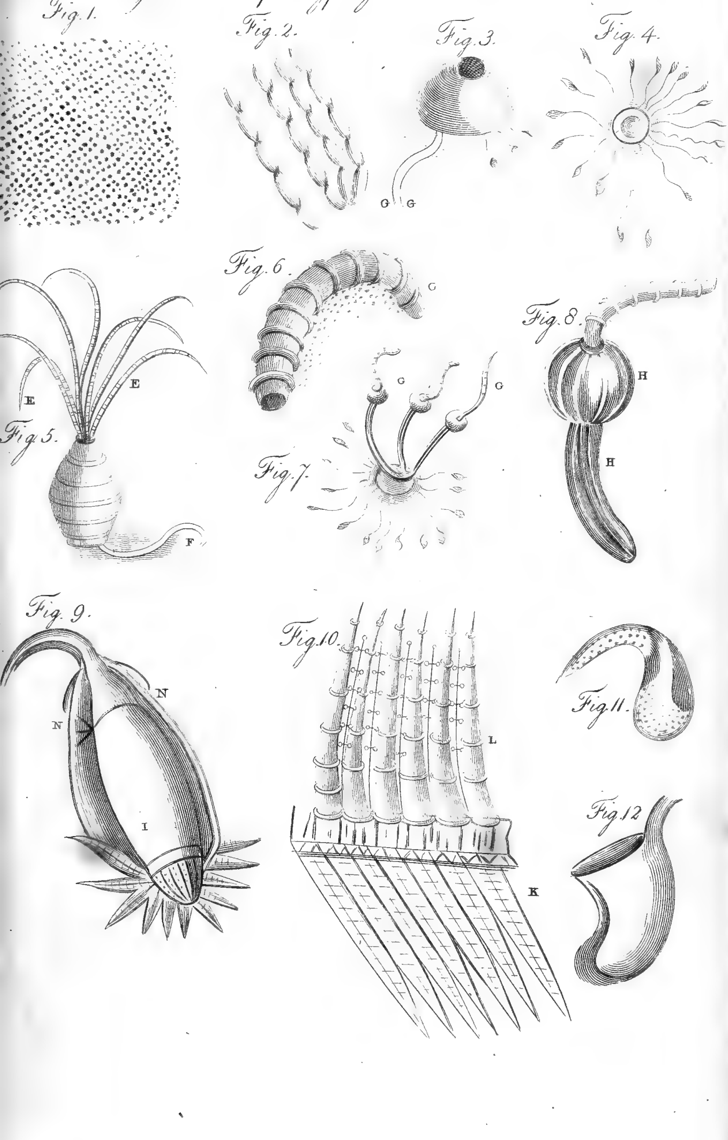
Blue calcareous stone from Vesuvius.

Lime	58
Carbonic acid	28·50
Ammoniacal water	11
Magnesia	0·50
Oxide of iron	0·25
Charcoal	0·25
Silex	1·25
	99·75

(To be concluded in a future Number.)

* Journ. vol. xxvii, p. 152. † Ib. ‡ Ib. p. 154.

Dissections of Cryptogamian Plants.



Dissections of Cryptogamian Plants.

Fig. 1.

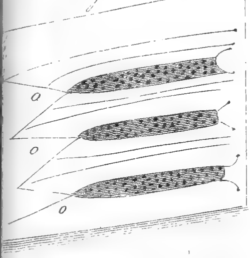


Fig. 2.

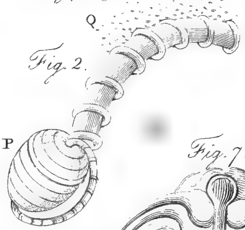


Fig. 3.

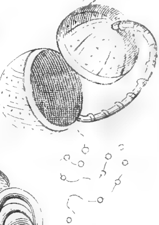


Fig. 7.

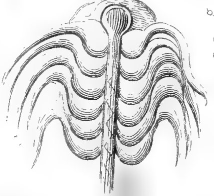


Fig. 4

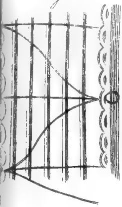


Fig. 5.

Fig. 6.

Fig. 8.

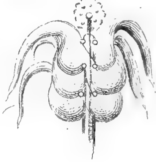


Fig. 9.

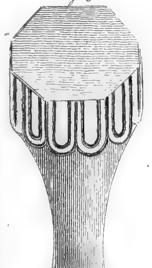


Fig. 10.

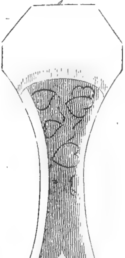


Fig. 11.

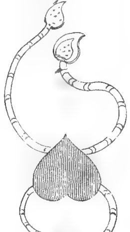


Fig. 12.

Mr. E. Sydiatt's Smicrologometer.

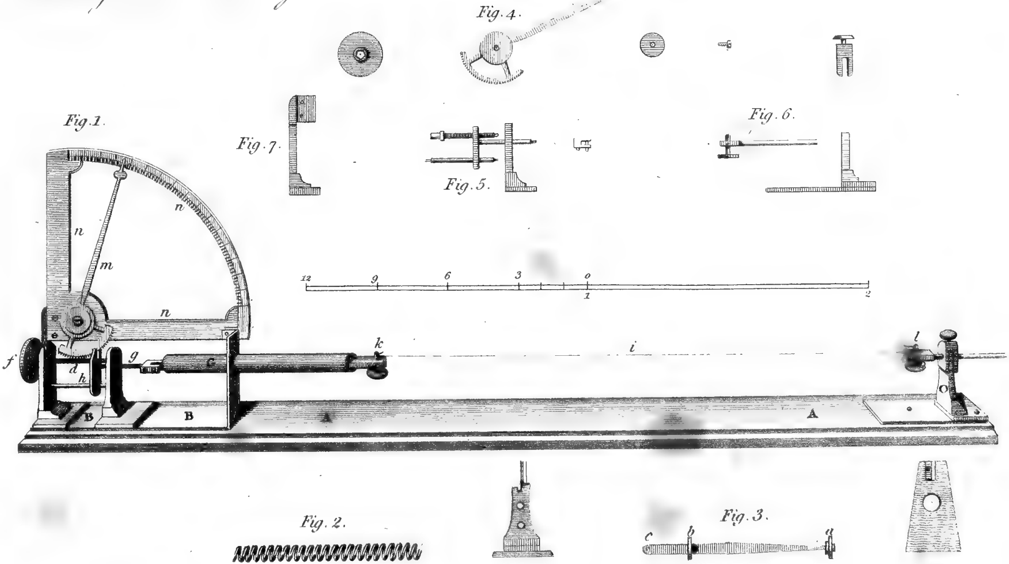


Fig. 3.

Fig. 6.

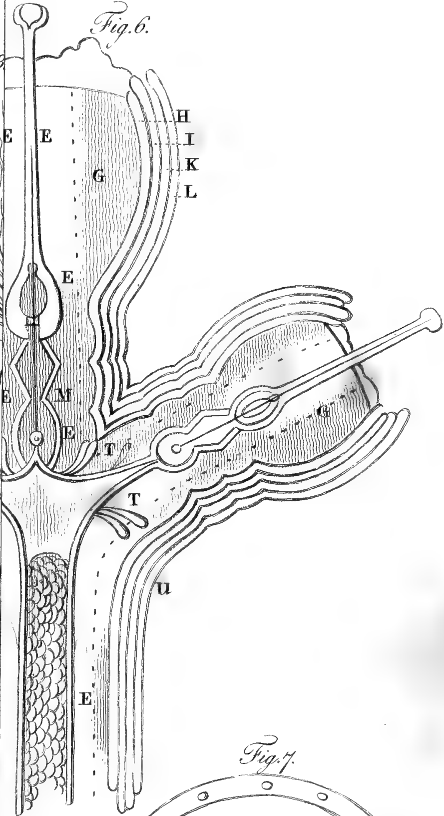


Fig. 1.

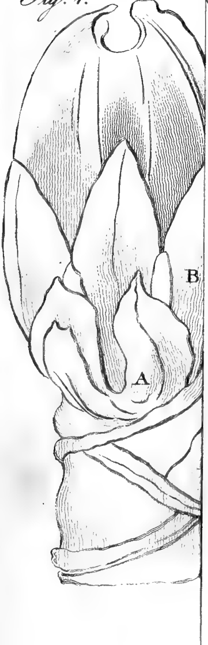
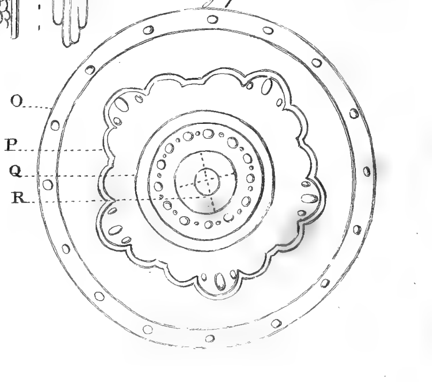
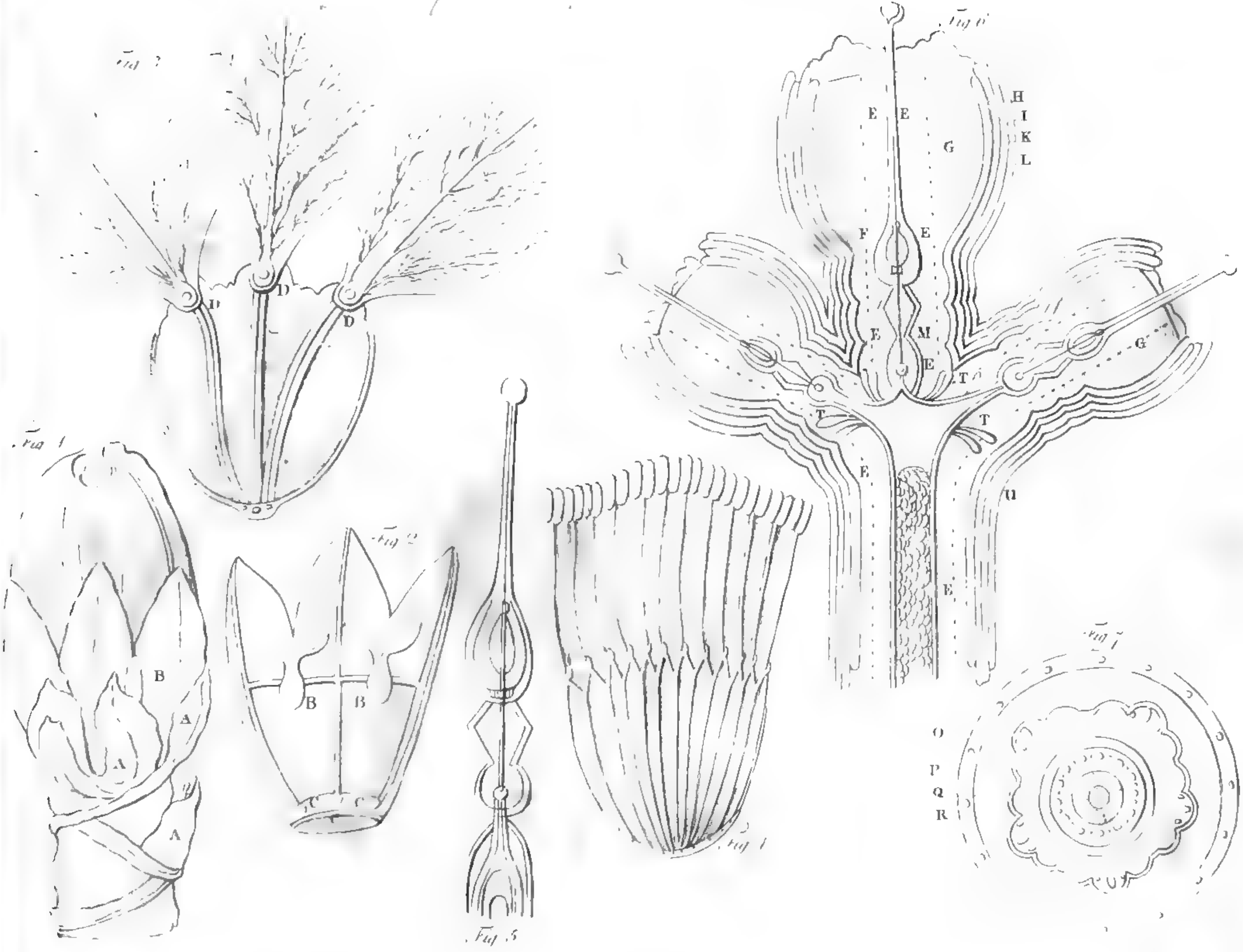


Fig. 7.



Dissections of a Flower, by M.rs C. Ibbetson.



Dissection of a branch of Laburnum by Mrs. A. Ibbetson.

Fig. 1.

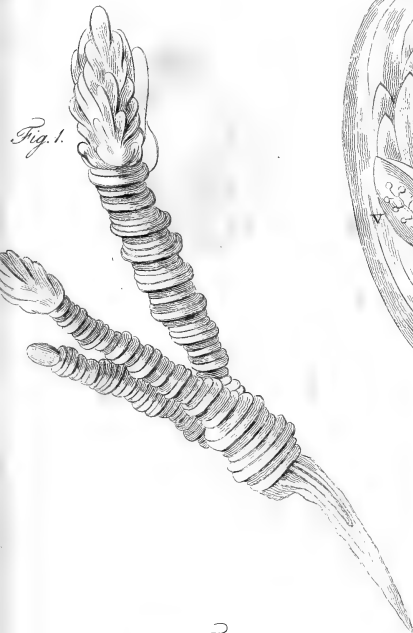


Fig. 2.

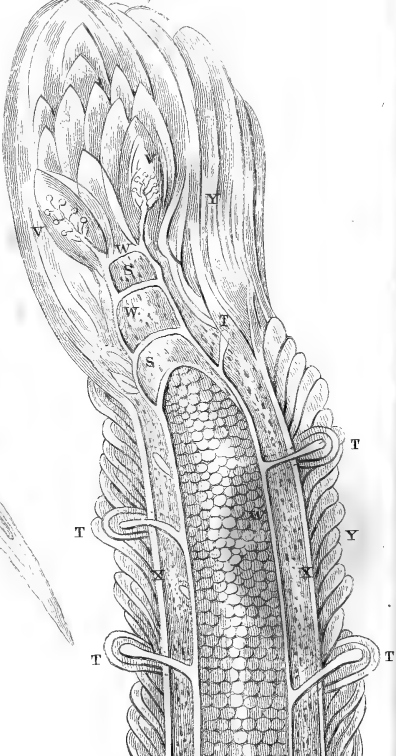
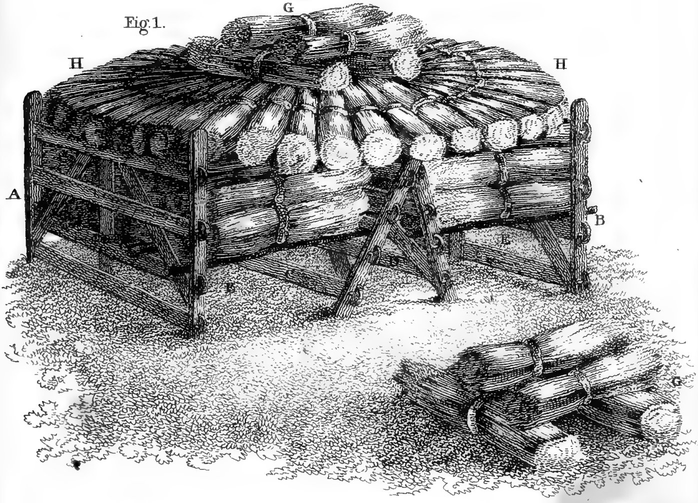


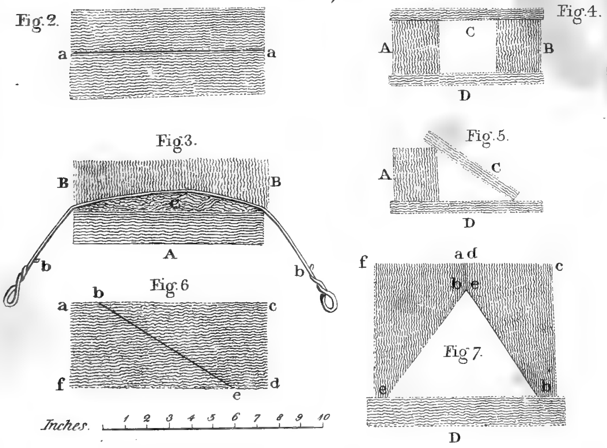
Fig. 3.

Foundation of Mr. Wm. Jones's Temporary Corn Rick

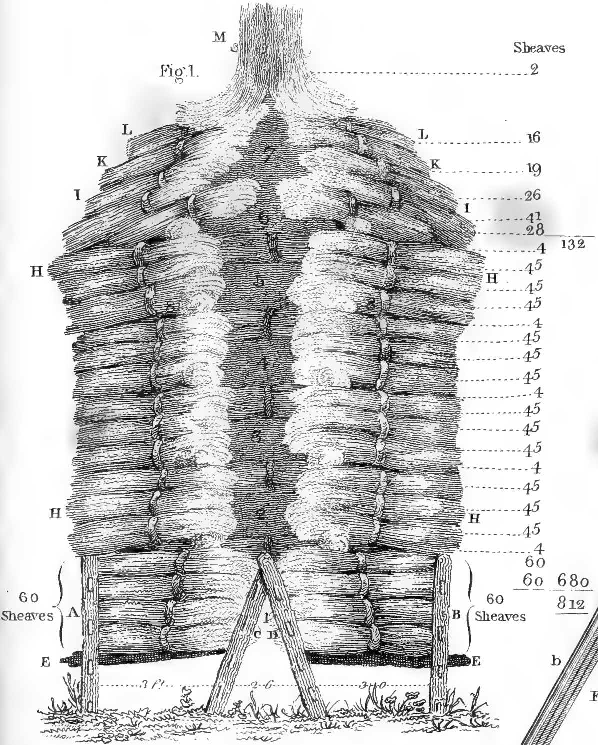
Fig. 1.



Mr. Stephens's Method of dividing Bricks



Section of Mr. Wm. Jones's temporary Corn Rick.



Mr. Waistell's Improvement on the Dibble, or Tool for planting Acorns in Bushes.

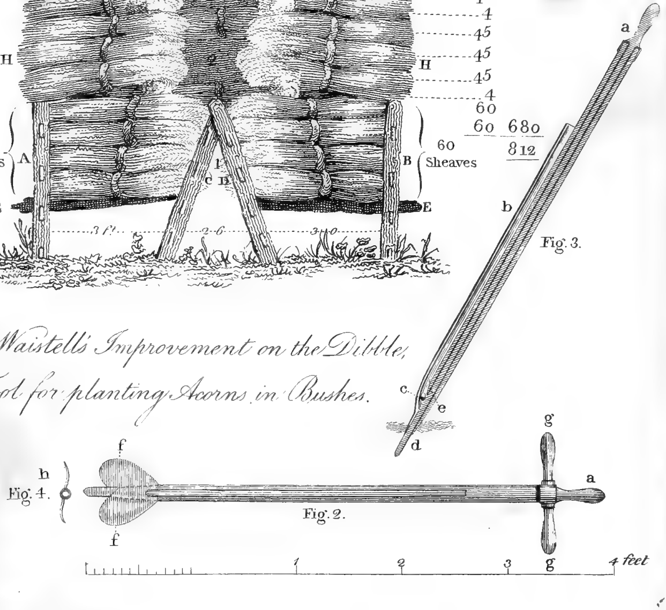


Fig. 1.

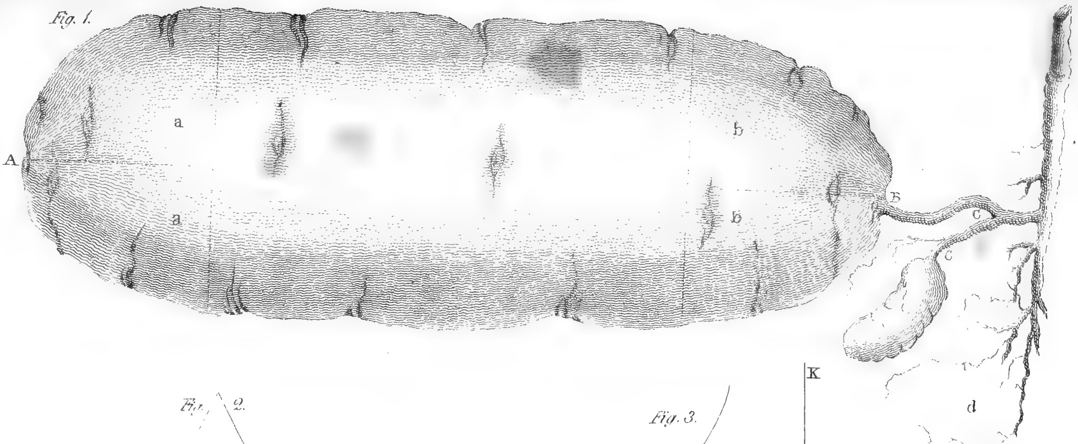


Fig. 2.

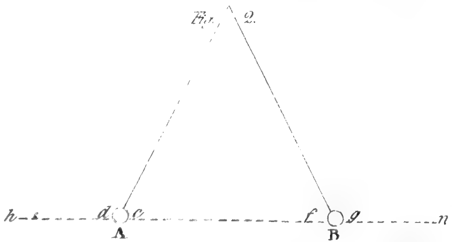
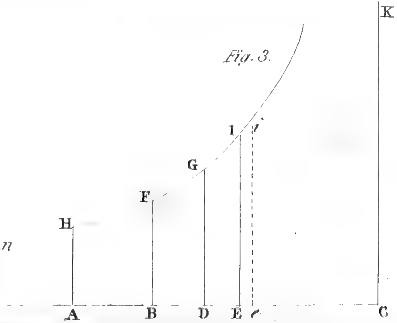


Fig. 3.



Improvement in Fowling Pieces.

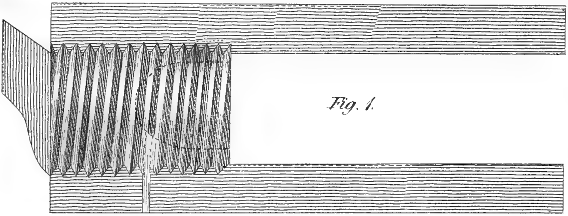


Fig. 1.

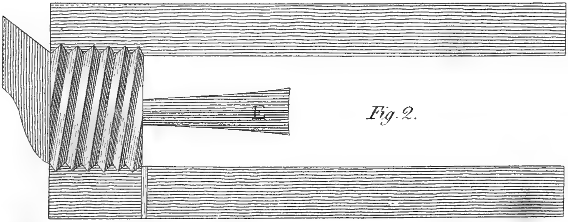


Fig. 2.

Mr. John Fuller's Improved Scarificator.

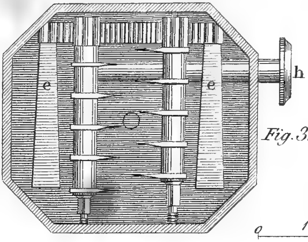


Fig. 3.

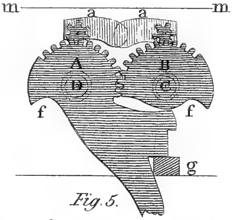


Fig. 5.

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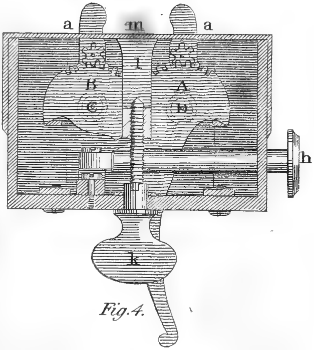

Fig. 4.

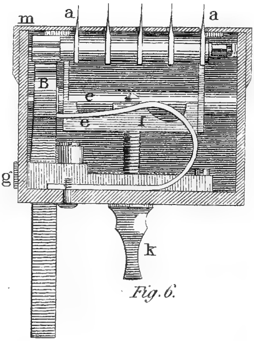

Fig. 6.

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